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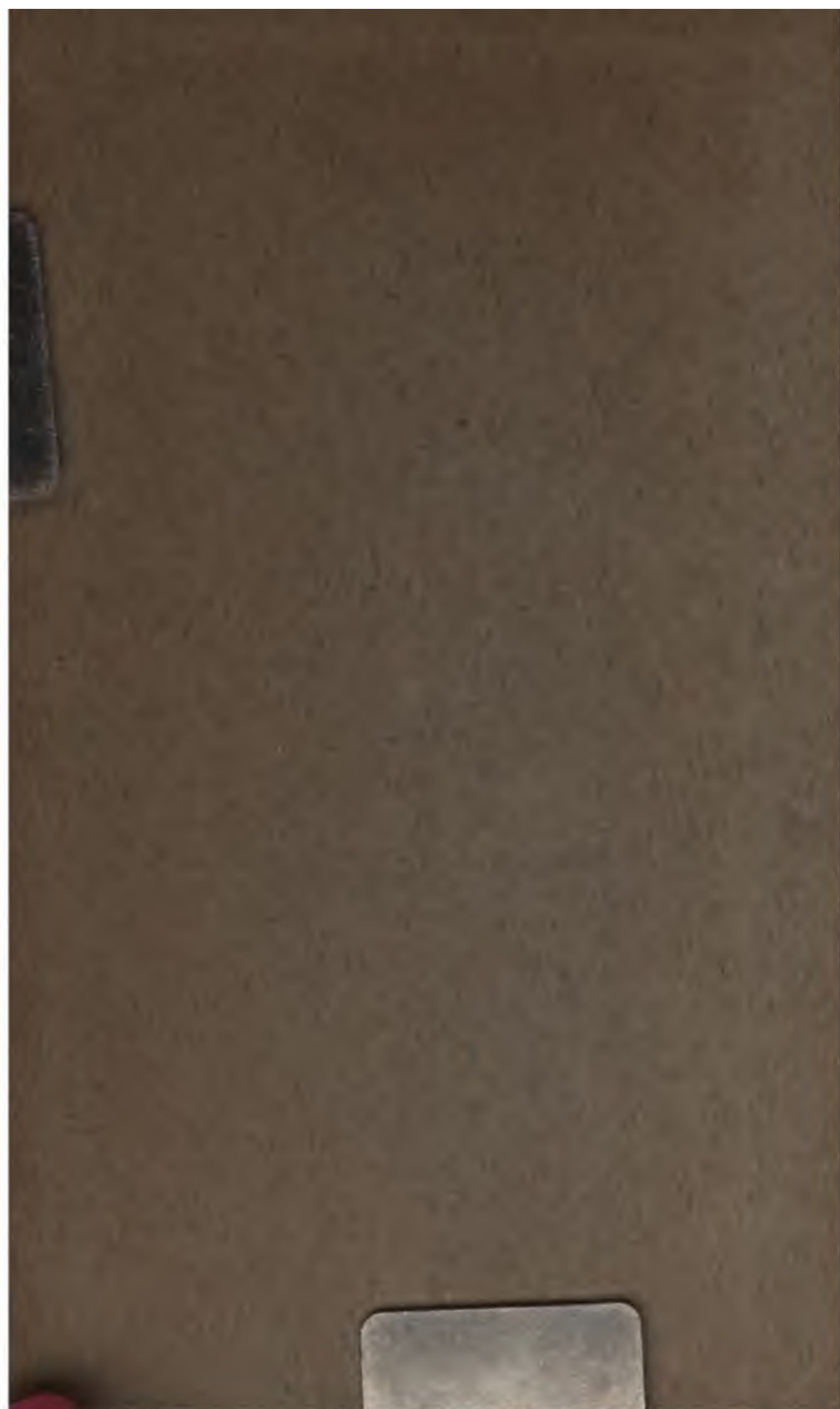
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A
JOURNAL
OF
NATURAL PHILOSOPHY,
CHEMISTRY,
AND
THE ARTS.



VOL. I.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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1802. - 1813

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PAGE: 00000001

PREFACE.

THE conclusion of the first Volume of the new Series of this Journal, demands that I should return my sincerest acknowledgments to those excellent Correspondents who have assisted me in improving it, and to the Public at large, who have shewn by the increased demand, that they approve the alterations which have been made.

Among the advantages I will here mention only two; the first by no means inconsiderable, is the very neat and correct typographical execution, which would be honorable to the Printer of a complete copy of a work at leisure, and is singularly valuable in a periodical work;—the other, namely the side notes, requires so much diligence and expence, that it is likely they will long continue to be peculiar to this Journal. These circumstances, added to the unvaried fidelity of conduct in quotation and regard to the rights of others; from which the Editor never has nor will depart, have naturally led men of eminence to chuse this Journal as the repository for their communications.

It is with a very high degree of satisfaction that I can state, that half the materials contained in the present Volume are original, and avowed by their very respectable Authors; that one third part consists of translations, or abstracts of excellent works never before published in English, and that the remaining sixth part are select Memoirs, either abridged or extracted from the Transactions of the Royal Society, and other recent and authentic sources of discovery.

The Authors of original Papers are Doctors Thomson, Beddoes, Higgins, and Priestley; the Rev. W. Pearson; Messrs. Gough, Close, Davy, Murray, R. L. Edgeworth, Stodart, Zach, Accum, Ez. Walker, Goodwyn, W. Walker, and W. N.—Of foreign works, Thenard, Peres, Tromsdorf, Prevost, Dolomieu, Prout, Loysel, Haüy, Volta, Van Marum, Le Blanc, Carradori, Darcet, Parmentier, Vauquelin, Darracq.—And of English Memoirs abridged or extracted, Mendoza, Herschel, Chenevix, Davy, Ware, Cooper

PREFACE.

Cooper, Englefield, Delafons, Sheldrake, Capper, Arkwright. For the various and important objects of their research, he must refer to the Table of Contents, the ample Index, and the other assistant indications he has been sedulous to present to the Reader.

This Volume contains sixteen Copper Plates, illustrating nineteen distinct objects : viz. Herschel's discoveries of the Structure and Changes of the Sun. 2. Various circular Instruments for measuring Angles, 3, 4. Two Hydraulic Engines invented by Mr. Close for raising water above its Level by the Syphon. 5. Method of making Gun Flints. 6. Mr. Cooper's Designs to shew by the structure of the Ear, that a species of Deafness may be cured by puncture of the Membrane of the Tympanum. 7. Loysel's new Apparatus for bleaching Paper. 8. Carangeau's Instrument for measuring Crystals. 9. Dr. Thomson's Apparatus to shew that the Rumfordian Circulation of Solids in Fluids is not occasioned by Currents. 10. Volta's Sketches in support of his Theory of Galvanism. 11. Trevithack's new and powerful Engine for producing Mechanic Force by an included Column of Water. 12. Mr. Murray's Apparatus for new Experiments on the conducting Power of Fluids. 13. Dr. Herschel's Apparatus for viewing the Sun through Fluids. 14. Mr. Hornblower's Hydraulic Bellows for a Forge. 15. Rev. Wm. Pearson's Projection of the Orbits of the Earth and of the new Planet Ceres, 16. Mr. Murray's Apparatus for shewing the conducting Power of Mercury and of Oil in a Vessel of Ice. 17. A simple and free Escapement for Time Pieces, by Mr. Delafons. 18. Mr. Arkwright's Engine for raising Ores from Mines. And, 19. A cheap, simple, and accurate perspective Instrument, by Richard Lovell Edgeworth, Esq.

With prospects already so animating, under the favorable circumstances of a general Peace, and the facilities it affords to philosophical pursuits and manufacturing inventions, it is to be expected that the value of our communications and discoveries will increase.

Soho Square, London, May 1, 1802.

TABLE OF CONTENTS

TO THIS FIRST VOLUME.

JANUARY 1802.

ENGRAVINGS of the following Objects: 1. Dr. Herschel's Discoveries of the Constitution of the Sun; 2. Circular Instruments by Reflection of Mayer, Borda, and Mendoza; and, 3, 4. Two Hydraulic Engines by Mr. Clofe, in which the Syphon is rendered effective in raising Water above its Level for an unlimited Time.

I. On the Exhibition of a Series of Primes, and the Resolution of a Compound Number into all its Factors. By Mr. J. Gough.	Page 1
II. On an improved Reflecting Circle. By Joseph de Mendoza Rios, Esq. F. R. S.	4
III. Observations tending to investigate the Nature of the Sun, in order to find the Causes or Symptoms of its variable Emission of Light and Heat; with Remarks on the Use that may possibly be drawn from Solar Observations. By William Herschel, LL. D. F. R. S.	13
IV. Observations and Experiments upon Dr. James's Powder, with a Method of preparing, in the humid Way, a similar substance. By Richard Chenevix, Esq. F. R. S. M. R. I. A.	22
V. Construction of an Hydraulic Apparatus, which by Means of the Syphon raises Water above its Level, and performs its Alternations without Attendance. By Mr. Wm. Clofe.	27
VI. Outline of the Properties and Habitudes of the Metallic Substance, lately discovered by Charles Hatchett, Esq. and by him denominated Columbium.	32
VII. On the Sebacic Acid, or Acid of Fat. By Cit. Thenard.	34
VIII. Improvement in the Art of preparing Radical Vinegar. By Cit. Y. Peres, Jun.	39
IX. An Account of a new Eudiometer. By Mr. H. Davy, Director of the Chemical Laboratory, and Lecturer on Chemistry to the Royal Institution.	41
X. Analysis of a newly discovered Mineral resembling the Hyacinth. By Professor Tromsdorff.	44
XI. Philosophical Disquisitions on the Processes of common Life—Art of Shaving—W. N.	47
XII. Abstract of the Enquiries of Cit. Benedict-Prevost, and some other Natural Philosophers, relative to the Motions of Odorous Substances placed upon Water. By Cit. Biot.	51
XIII. Case of a young Gentleman, who recovered his Sight when seven Years of Age, after having been deprived of it by Cataracts, before he was a Year old; with Remarks. By Mr. James Ware, Surgeon.	57
Scientific News. French National Institute.—New Planet.	69 to 72
Account of Books of Science. Transactions of the Royal Society.—Gregory's Economy.—Murray's Chemistry.—Capper on Wind and Monsoons.—J. G. W. on Beet Sugar.—Des Mortieres on Prussian Blue.—Socquet on Caloric.—Clanlare's Atlas of the South of Europe.—Memoirs of the Agricultural Society.	73 to 79
Books imported.	79, 80
VOL. I.—1802.	FEBRUARY.

FEBRUARY 1802.

Engravings of the following Objects: 1. The Method of making Gun-Flints; 2. Structure of the Human Ear; to illustrate the Cure of a Species of Deafness; 3. The new Apparatus for bleaching the Materials of Paper. And, 4. The Graphometer for measuring Crystals; Dr. Thomson's Apparatus for ascertaining whether Fluids move by change of Temperature; and some Apparatus for shewing the Electricity afforded by Contact of Metals.	
I. On the supposed Currents in hot Liquids. By Thomas Thomson, M. D. Lecturer on Chemistry in Edinburgh. From the Author.	Page 81
II. A Memoir on the Art of making Gun-Flints. By Citizen Dolomieu.	88
III. Letter from Dr. Beddoes on Prognostics of the Weather, the Effects of the Nitrous Oxide, and other Objects.	98
IV. On the Quantity of Nutriment to be obtained from various Kinds of Bones, and the best Method of extracting it, by Professor Proust.	100
V. Observations on the Effects which take Place from the Destruction of the Membrani Tympani of the Ear; with an Account of an Operation for the Removal of a particular Species of Deafness. By Mr. Astley Cooper.	102
VI. Note respecting the Absorption of Nitrous Gas, by Solutions of Green Sulphate and Muriate of Iron. By Mr. H. Davy, Director of the Laboratory, and Lecturer on Chemistry to the Royal Institution.—(From the Author.)	107
VII. On certain Metallic Sulphurets. By Proust, Professor of Chemistry at Madrid	109
VIII. A Memoir on the Method of Bleaching the Paste of Paper. By Cir. Loyel.	118
IX. On the Sound produced by a Current of Hydrogen Gas passing through a Tube. With a Letter from Dr. Higgins, respecting the Time of its Discovery.	129
X. Description of the Graphometer, or Instrument of Cit. Carangeau, for measuring the Angles of Crystals.—From the Mineralogie of Cit. Haüy.	132
XI. Letter of Professor Volta to J. C. Delamethrie, on the Galvanic Phenomena.	135
XII. An Account of a Method of Constructing Simple and Compound Galvanic Combinations, without the Use of Metallic Substances, by Means of Charcoal and different Fluids. By Mr. Davy.	144
XIII. Short Analysis of the Principles and Structure of Mr. Cloze's Hydraulic Engine—W. N.	145
XIV. Simple Process for taking a Copy of a Recent Manuscript. Communicated to the Philomathic Society at Paris. By Charles Coquebert.	147
XV. Narrative and Explanation of the Appearance of Phantoms, and other Figures in the Exhibition of the Phantasmagoria. With Remarks on the Philosophical Use of Common Occurrences.—W. N.	ib.
Scientific News. Extract of a Letter from Doctor Lorenz De Crell.—Corrindon or Adamantine Spar found near Philadelphia.—Nature of the Earth which is eaten by the Inhabitants of New Caledonia.—Account of a blue Oxide of Iron.—Use of Whiskers in certain Quadrupeds.—Galvanic Charge of a large Battery by the Metallic Pile.	151 to 154
Account of Books of Science. Davy's Syllabus of a Course of Lectures on Chemistry—Kelly's Elements of Book-keeping—Memoir sur l'Influence de l'Air, &c.—Memoirs d'Agriculture, &c.	155 to 159
Books imported	160

MARCH

CONTENTS.

iii

MARCH 1802.

Engravings of the following Objects: 1. A new and powerful Engine, which acts by the alternate or double Pressure of a Column of Water on each Side of a Piston. By Mr. R. Trevithack. 2. Dr. Murray's Apparatus for new Experiments on the Transmission of Heat through Fluids. 3. Dr. Herschell's darkening Apparatus to view the Sun through Fluids. And, 4. Hydraulic Bellows for a Smith's Forge. By Mr. Hornblower.	
I. Description of an Engine which operates by the Pressure and Descent of a Column of Water against a Piston; nearly in the same Manner as the double Steam Engine operates by Means of Steam. Communicated by the Inventor Mr. Richard Trevithack, of Camborne, near Truro, in Cornwall. Page 161	
II. Experiments and Remarks on the Passage of Heat through Fluids downwards, particularly with Regard to the Uncertainty produced by the Vessel; with a Method of obviating that Uncertainty altogether. By John Murray, M. D. Lecturer on Natural Philosophy and Chemistry at Edinburgh. Communicated by the Author	165
III. Letter from Dr. Van Marum to Mr. Volta, Professor at Pavia, containing Experiments on the Electric Pile, made by him and Professor Pfaff, in the Teylerian Laboratory at Haarlem, in November, 1801	173
IV. A Reply to Mr. Cruickshank's Observations in Defence of the New System of Chemistry, in the Philosophical Journal. By Joseph Priestley, L. L. D. F. R. S. &c. Communicated by the Author	181
V. A Statement of the Experiments made by the Rev. Abraham Bennet, F. R. S. on the Electricity produced by the contact of Metals previous to the Year 1789, and also of these made by Mr. Tiberius Cavallo, F. R. S. previous to the Year 1795, to which Allusion was made at Page 144 of this Journal. —W. N.	184
VI. On the Formation of Crystals, describing a Method of producing them large and regular. By Cit. Le Blanc	191
VII. Accounts of the New Planet Ceres	193
VIII. Observations and Experiments relating to the Pile of Volta. In a Letter from Joseph Priestley, L. L. D. F. R. S.	193
IX. Experiments and Observations towards determining the Influence of Oxygen on Germination. By Dr. Carradori	204
X. On the Choice and Use of a Razor. By a Correspondent	210
XI. Observations on the Method of painting with Milk. By Cit. Darcet, Member of the Lyceum of Arts, and Essayer of Money	212
XII. Description of an Hydraulic Bellows for a Smith's Forge. By Mr. J. C. Hornblower	219
XIII. On the Practicability and Advantage of a general System of Rail-Roads, and the Means of carrying the same into Effect. In a Letter from Richard Lovell Edgeworth, Esq.	219
XIV. Description of the darkening Apparatus for Telescopes by Means of Fluids. By William Herschel, L. L. D. F. R. S.	217
XV. On the Effects of the Respiration of the Nitrous Oxide; particularly an Instance in which the Excitation of the System produced unpleasant Symptoms. In a Letter from Mr. James Stodart	225
XVI. Remarks on the Processes for clarifying Liquids. By Cit. Parmentier	228
Scientific News. Extract of a Letter from Brunn, in Moravia—On the Muria-tic Acid—A new Method of obtaining the Gallic Acid pure—Extracts of a Letter of Cit. Vauquelin to Cit. Van Mons	234 to 233
Account of Books of Science. Murray's Elements of Chemistry—Young's Syllabus of a Course of Lectures on Natural and Experimental Philosophy—Mr. Blair's Popular Lectures	239, 249

APRIL

APRIL 1802.

Engravings of the following Objects: 1. The Rev. Mr. Pearson's Projection of the Orbits of the Earth, and the new Planet Ceres. 2. Apparatus by which Mr. Murray has shewn that Fluids conduct Heat downwards, when the Vessel is effectively a Nonconductor. 3. Mr. De Lafons's free Escapement for Time Pieces. 4. Mr. Arkwright's Engine for raising Ores from Mines. And, 5. A cheap, simple, and accurate perspective Instrument, by R. L. Edgeworth, Esq.	
I. Experiments on the Transmission of Heat downwards through Mercury and through Oil contained in Vessels of Ice; by which those Fluids are proved to be proper Conductors of Heat. By John Murray, Lecturer in Chemistry, Materia Medica, and Pharmacy, at Edinburgh. Communicated by the Author.	Page 241
II. Description of a new Escapement for Watches, invented by Mr. John De Lafons.	251
III. Remarks on the Processes for clarifying Liquids. By Citizen Parmentier.	253
IV. On the Discovery of the Arseniates of Copper and of Iron. By a Correspondent.	258
V. On the Nature and Preparation of drying Oils; with a View to the Improvement of such as are used by Artists, as Vehicles for Painting. By Mr. Timothy Sheldrake.	259
VI. On the Native and Artificial Sulphurets of Iron. By Professor Proust.	269
VII. General Observations on the Causes which influence the Weather in England, and the popular Methods of judging of the Weather. By James Capper, Esq. formerly Colonel and Comptroller General of the Army and Fortification Accounts on the Coast of Coromandel.	275
VIII. Description of a cheap, simple, and portable Instrument, for determining the Positions of Objects in taking a Picture from the Life. By R. L. Edgeworth, Esq.	281
IX. Concerning the new Planet Ceres. In a Letter from the Rev. William Pearson, including an Extract of a Letter from the Baron De Zach to Mr. Edward Troughton.	284
X. Entertaining chemical Experiments; with Notices of various new Facts and Observations respecting the Products of Nature and Art. By Mr. Frederick Accum. Communicated by the Author.	295
XI. The Method of crystallizing Lime. By Tromsdorff.	302
XII. On the Crystallization of the Hydrosulphuret of Soda. By Cit. Vauquelin.	303
XIII. Memoir respecting a new Combination discovered in Zaffre, which Cit. Brugnatelli supposed to be a peculiar Acid, which he denominated the Cobaltic Acid. By Cit. Darraq.	304
XIV. On the Plumb Line and Spirit Level. By Ezekiel Walker. Communicated by the Author.	309
XV. Description of a Machine for raising Ore from Mines. By Mr. T. Arkwright, of Kendal, Westmoreland.	313
XVI. Curious Properties of Prime Numbers, taken as the Divisors of Unity. By a Correspondent.	314
Scientific News. Extract of a Letter from Mr. W. Walker, Lecturer in the Eidouranian, on the new Planet Ceres.—Extract of a Letter from M. Zach, Director of the Observatory of Gotha, to C. Mechain, of the Institute, Administrator of the Observatory at Paris.—Spiders' Webs to form the cross Wires in the Eye Piece of Astronomical and other Instruments.—Thick Iron burned in Oxygen.—Extreme Accuracy of tuning Musical Instruments.	317 to 320

ERRATA.

I have observed no Errata by which the Reader can be misled, but must remark, that the table at page 73, called *Observations* of the Ceres is wrong intitled: the table being merely an ephemeris, calculated for re-discovering the planet from the former observations.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JANUARY, 1802.

ARTICLE I.

*On the Exhibition of a Series of Primes, and the Resolution of a
Compound Number into all its Factors. By Mr. J. GOUGH.*

To Mr. NICHOLSON.

SIR,

M. EULER remarks in his Algebra, that mathematicians have not yet discovered a way of exhibiting a natural series of primes; and in reality I know of no writer on arithmetic, who shews how a compound number may be resolved into all its factors, otherwise than by trial. The following method, however, may be said to solve both problems, perhaps in as ready a way as the nature of numbers will admit; and on fixed principles: it depends upon the following property of compound numbers.

If a compound number, which is not a square, be resolved into any two of its factors, one of these factors will be less than the root of the next greater square, and the other will be equal to, or greater than the same root.—If you deny the property, then both factors must be either greater than the root of the square, in which case the product will be greater than the square; or, they must be both less, than that root, on which

VOL. I.—JANUARY.

B

supposition

Natural series of
primes and reso-
lution of a num-
ber into its fac-
tors not yet ef-
fected.

Property of
compound num-
bers.

supposition the product will not be greater than the next less square, seeing the factors are whole numbers. Now suppose $f^2 - c$ to represent any number whatever, where f^2 is the next greater square, and c the excess of that square above the given number: then any two factors of $f^2 - c$ may be expressed by $f - p$, and $f + p + 2n$, from whence the following method of determining all the factors to two successive squares, and their intermediate numbers, is easily derived.

Method of determining factors.

In a perpendicular column at the left hand of your future work, (*vide Tab. 1st.*) which exhibits the divisors of all the numbers from 49 to 36, write down the arithmetic progression 1, 2, 3, &c. beginning at the top with unity, and ending with f ; let all these numbers successively represent the divisor $f - p$, and corresponding to each of them, draw a double horizontal column: when $f - p = 1$, make c in the uppermost horizontal column $= 2f - 1$, $3f - 2$, $2f - 3$, &c. to 0, because $2f - 1$ is the difference of the two squares in all cases; and for $f + p + 2n$, write $\sqrt{f^2 - 1^2}$, $\sqrt{f^2 - 2^2} + 1$, $\sqrt{f^2 - 3^2} + 2$, &c. to f^2 . In order to fill up the other columns, divide f^2 by the respective value of $f - p$, and place the remainder, if there be any, in the upper part of the horizontal column, perpendicularly under the same value of c in the highest column; but if there be no remainder, place a cypher in its room: then increase this remainder as often as you can by the addition of $f - p$, not to make it exceed $2f - 1$, and place these, each directly under the same number in the highest column, and write c on the left hand.

Under the first remainder or cypher, place the quotient $\frac{f^2}{f - p}$ in whole numbers; from which, subtract unity as oft as $f - p$ was added to the remainder, and call these $f + p + 2n$; but when $f - p = f$, put 0 alone for the value of c : when all the columns are filled up in this manner, the factors of any given number are thus found. Find the value of c corresponding to that number in the highest column; then find how often the same number recurs, by casting your eye perpendicularly down the columns, the factors will be the value of $f + p + 2n$, directly under that value of c in each column; and the corresponding value of $f - p$: consequently, these values of c which are only found in the highest column, indicate that the
number

8

But if a series of primes only is to be calculated, the work **Primes.** may be shortened, by omitting the values of $f + p + 2n$ in all the columns except the highest, and marking those numbers with asterisks which correspond to the values of c only found in the uppermost line, as in *Tab. 2d.* where the prime numbers from 1 to 16 are calculated.

Note, the values of c may be otherwise calculated:—from the product fp take the greatest multiple of f — p that can be subtracted from it; this will give the least value of c corresponding to that value of $f-p$.

Kendal, Dec. 7, 1801.

J. GOUGH.

TABLE I.

[illegible]

TABLE II.

From 1 to 4					From 5 to 9					From 10 to 16							
$f-p=1$	$c=$	3	2	1	0	4	3	2	1	0	6	5	4	3	2	1	0
		*	*	*		*		*				*		*			
	$f+p+2n$	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
$f-p=2$	$c=$				0		3		1		6		4		2		0
$f-p=3$	$c=$									0			4			1	
$f-p=4$	$c=$																0

II.

*On an improved Reflecting Circle. By JOSEPH DE MENDOZA
Rios, Esq. F. R. S. (Phil. Trans. 1801.)*

Advantages of
large instru-
ments in dimi-
nishing the er-
rors of division
and eccentricity.

Invention of
Mayer for dimi-
nishing the same
errors at plea-
sure in small
instruments by
reflection.

IN practical astronomy large instruments are useful, not only to enable the observer to read the angles to a small fraction of a degree, but likewise to diminish, in the construction, the inaccuracies which proceed both from the errors of the divisions and the eccentricity of the index. Frames of considerable dimensions admit also the application of telescopes with great magnifying powers, which is a circumstance of the utmost importance in celestial observations. As the reflecting instruments employed at sea are supported by the hand, their weight and scale are limited within a narrow compass; and it seemed very difficult to obviate, by any expedient, the inconveniences arising from the smallness of their size, while it was impossible to increase it. The celebrated Tobias Mayer contrived, however, a method to determine, at one reading, instead of the simple angle observed, a multiple of the same angle; and, by this means, the instrument became, in practice, capable of any degree of accuracy, as far as regards the above mentioned errors. His invention is essentially different from the mere repetition of the observations; and my object requires that I should explain the principle upon which it is founded.

Mr.

Mr. Mayer proposed to complete the limb of the sextant, *1. He made the limb an entire circle. And 2. the centre, with an additional index, which I shall call the horizon index, in order to distinguish it from the centre index, to which the centre glass is attached. This instrument is represented in Plate I. Fig. 1; and the manner of using it is as follows: after the index A is set at o, (the beginning of the divisions,) the two glasses are rendered parallel; as is usually practised with Hadley's quadrant, by moving the horizon index B, till the horizon of the sea, (or the sun, or any other object,) or its direct image, and the doubly reflected image of the same, seen through the telescope, coincide.*

After fixing the horizon index in that position, the centre index A is to be moved, in order to measure the distance of the two objects S and L, (which I shall suppose the sun and moon,) by bringing into contact the doubly reflected image of the sun with the direct image of the moon, seen through the telescope.

The centre image will then be at M, and the arch o M might give, as in the sextant, the angular distance required; but the construction of the circle renders it easy, in this position, to effect again the parallelism of the glasses, and to make another observation of the contact, in the like manner as from o;

which operation will bring the centre index to N. The index will then give o N, or double the distance; and, as it must be divided by 2, in order to have the angle required, the errors of division and eccentricity, which, together, I shall call the error of the instrument, will be likewise reduced to one half.

It is obvious, that by successive repetitions of the same process, triple, quadruple, &c. the distance may be obtained, and the said error further reduced, in the inverse ratio of the multiplication of the distance, to any degree of approximation required.

The method of rendering the glasses parallel, by means of the horizon of the sea, is not accurate, on account of the distinctness of the images; and, when the sun is used for that purpose, the observation becomes fatiguing to the eye. The repetition of that observation, by one or the other method, remained therefore a considerable inconvenience attached to Mr. Mayer's circle. The author himself seems to have been of that opinion, as he proposed to provide the instrument with a diagonal rule, fixed upon one of the indexes, so that the

limb an entire circle. And 2. he fixed the horizon glass upon an index moveable on the same centre, but independent of that which carries the glass called the index glass.

Use. Adjust the glasses parallel by the horizon index.

Observe the angle by the first index and fix it.

Make the glasses again parallel by the horizon index.

Observe the angle as before.

Adjust for parallelism, and repeat the observation.

And as this process may be continued at pleasure, and the first index will pass over the measure of the

angle as many times as there are repetitions; Divide the whole space by the number of repetitions, and the quotient will be the correct angle; the errors of the instrument being diminished in the other same ratio.

Objection. It is inconvenient to make the adjustments for parallelism. Happy invention of Borda for removing it.

By a slight alteration in the construction he renders it practicable to observe the same angle twice, by moving the index in contrary directions without shifting the face of the instrument;

which gives the double distance, and requires not any adjustment for parallelism.

other index should touch it when the glasses were parallel; but an adjustment of this nature must be subject to great errors, and was never adopted in practice. The Chevalier de Borda, wishing to remove that imperfection, had the happy idea of rendering the parallelism of the glasses unnecessary, by substituting the observation of the angular distance of the two objects, to that of the coincidence of the images of the same object. This constitutes the second great improvement of the reflecting circle, which it is necessary for me to explain, before I proceed to the account of my own investigations*.

In Borda's circle, the telescope is fixed at some distance from the centre, and the horizon glass is carried near the border of the instrument, as in Plate I. Fig. 2. By this arrangement, the rays of light can arrive at the centre glass, both from the heavenly bodies situated to the right of the horizon index, as S' , and from those situated to the left, as S . Thus, if the glasses are parallel to one another, when the centre index is at o , it is obvious that there are two ways of making the observations. While the direct image of the moon L is seen through the telescope, the angular distance to the sun, if at S , may be measured by moving the centre index to m , in order to produce the contact; or, if the sun is at S' , the same operation may be performed, by using the contrary motion to m' . The first kind of observation, the Chevalier de Borda calls *observation to the left*; and the second, *observation to the right*. Suppose now, that (the horizon index being fixed in the same position) the distance from L to S is observed to the left, by bringing into contact the doubly reflected image of S with the image of L , seen without reflection; let us then turn the instrument round, keeping it in the same plane, so as to have the direct image of S through the telescope, and thus make an observation of the same distance to the right; the position of the centre index being in the first observation at m , and in the second observation at m' , it is clear, that if o is the point where the parallelism of the glasses takes place, om is equal to om' ; and, that the arch mm' , determined by the two positions, will give double the distance.

* It does not belong to my present plan, to explain the effect of Borda's improvement in correcting the errors which arise from the want of parallelism in the surfaces of the glasses. This will be fully considered in another Paper, where I intend to give an account of several investigations which I have made upon that particular subject.

It

ON AN IMPROVED REFLECTING CIRCLE.

It will be more convenient to have the centre index at o , Convenient manipulation. when the first observation is made, in order to take the double distance at one reading, after the second observation. For this purpose, the first part of the process may be inverted, by previously fixing the centre index at the beginning of the divisions, and moving the horizon index H towards o , instead of moving the centre index A to m , or towards H .

The last kind of observation, in which the incident ray, This is denominated the crossed observation. which produces the first image upon the centre glass, may be conceived to run double the angular distance, passing in its way over the line of collimation, has been called, by the Chevalier de Borda, *the crossed observation*.

The same process may be repeated, by fixing alternately It may be repeated at pleasure. one of the indexes, and moving the other, and continuing successive sets of observations; each set of two crossed observations, one to the right and another to the left. The angle given by the instrument, will be equal to double the angular distance multiplied by the number of sets observed, or, in other terms; to the angular distance multiplied by the number of observations, which are always supposed to be made by pairs; an odd observation being of no value in this manner of using the circle.

I have expressed myself as if the observations could always And will not require a direct view of the brighter object, if the plane of the instrument be reversed at the second observation. be made by looking alternately at each object through the telescope, in order to bring into contact the doubly reflected image of the other object. This is not the case in the observations of the distances from the moon to the sun, or a star; it being then indispensable to compare, by reflection, the brightest of the two heavenly bodies; but there is a very easy method of obviating that inconvenience. After the contact of the images of S and L is observed, with the telescope directed to L , the position of the plane of the instrument may be inverted, turning it round the axis of vision OBL ; the incident ray will then answer to the point S' , equally distant from L as S , and the crossed observations will still give SS' , or double the distance.

Whether a circle is used simply, as Mayer proposed it, or according to Borda's method, its peculiar advantages chiefly depend on the multiplication of the distance required. I have therefore turned my attention to the improvement of this principle; and, with that view, I have contrived the construction which I am going to describe.

In

ON AN IMPROVED REFLECTING CIRCLE.

The author's improvement consists in applying an entire moveable circle, called the *flying nonius*, within or upon the divided circle. Each of these circles may be attached to either the center index or horizon index by the pressure of a clamp.

In the crossed observations made with Borda's circle, the indexes move alternately through an arch which in the divisions, is equal to double the distance: for example, the centre index comes, in the first crossed observation, from m to m' ; in the third crossed observation, from m' to m'' , &c. and the horizon index, in the second crossed observation, to h : in the fourth crossed observation, to h'' , &c. and, by each of the two indexes may be found the same multiple of the distance required. Let us now place the Nonius in a circle moving round the centre, over, or adjacent to, the usual limb which contains the divisions: it will easily be conceived, that, by attaching that circle, which I shall call the *Flying Nonius*, alternately to each of the indexes, it will serve as Nonius for both; and that, after any number of observations, it will give the compound motion of the two indexes. Thus, after the first observation, the *Flying Nonius* will, at each crossed observation, advance double the distance over the divisions, while each separate Nonius, fixed on the indexes, requires a set of two observations, to produce the same effect in Borda's circle.

View and description of the improved reflecting circle.

Plate I. (XXX. XXXI. and XXXII.) exhibits a perspective view, (a plan, and a section,) of the instrument, which, for the sake of distinction, I shall call my *Improved Reflecting Circle*. (The last Plate is particularly intended to shew the compound handle, which I have adapted to the instrument, in order to hold it with convenience and ease in every position *). These three

* The use of Mayer's circle, or of Borda's, as constructed till now, with only one handle attached to the centre, is extremely inconvenient in several positions, and particularly when it must be kept inverted downwards during the observation. For this reason, I thought it of importance to contrive such a support as would enable the observer to hold the instrument with the same ease in every direction. This is effected by means of the compound handle, attached to the horizon index, by the brace V and screw X, (Plates XXX. XXXI. and XXXII.) which index turns round the centre with the handle. When Mr. Troughton began to construct this sort of instrument, I recommended to him this improvement, which he has adapted to his reflecting circle.

I shall observe here, that Mr. Troughton's circles are not of the kind which I have endeavoured to improve. The scheme of his construction may be said to consist in completing the limb of a sextant

three representations, in which the same parts are marked with the same letters, are sufficient to give an accurate idea of the arrangement of the whole, and make it unnecessary for me to enter now into a minute detail of the mechanism of the apparatus. I therefore shall content myself with adding here only what concerns the general use, and the peculiar properties, of this instrument.

M is the divided limb of the circle, and N the Flying Nonius, to each of which the horizon index may be occasionally attached, by means of the clamps D, C; as well as the centre index, by means of the other clamps A, B. The peculiar property of the instrument being that of giving double the distance, I have thought proper to divide the circle into 360 degrees, and not into 720 according to the nature of the sextant. Thus, after a crossed observation, the reading of the Nonius will, without reduction, exhibit the measure of the simple distance. I have likewise extended the nonius round the circumference, so that, by the coincidence of two divisions, the number of degrees will appear on the limb, and that of the minutes and seconds on the flying nonius. The manner of making the observations with this instrument is as follows:

Adapt the 0 of the flying nonius to 360° of the limb; and then fasten the two clamps A, B, of the centre index * E E, by which the divisions will be kept in the same relative situations. Then, turn round the horizon index FF, and make an observation of the distance to the right. The contact must be adjusted by the screw G, the clamp C being fastened. The circle is divided into whole degrees, and the flying nonius has its divisions extended all round its circle.

tant to the whole circumference, and making it capable thereby of performing Borda's crossed observations, with as many noniuses as may be attached to the centre glass. But, Mr. Troughton's instrument is deprived of the principle invented by Mayer, for obtaining at one reading a multiple of the distance required; which is the great property of circles, and, in my opinion, the best means of diminishing their errors.

* Properly speaking, neither the centre nor the horizon indexes act, in this instrument, as such, both of them being deprived of the nonius, which is transferred to the flying circle; but, for the sake of perspicuity, I continue the use of those expressions, in order to distinguish the plates or rules which carry the centre and the horizon glasses.

Method of observing. Clamp the center index both to the limb and nonius (set with 0 opposite 360°). Observe an angle to the right by moving the horizon index; which then clamp to the nonius. Disengage the center index from the nonius, and repeat the observation to the left: the flying nonius will give the distance.

Leave

Leave this clamp fastened, and loosen the clamp A; thus turn the instrument, and make a crossed observation to the left, adjusting by means of the screw H, after having fastened the clamp D. At the end of this observation, the flying nonius will give the distance. Fasten now the clamp A, and loosen the clamps B and C, leaving the clamp D fastened; then turn the instrument again, and make a crossed observation to the right. At the end of this observation, the flying nonius will give double the distance. By successively inverting the use of the clamps, this alternate process may be continued *ad libitum*; and each crossed observation will increase the reading, by an arch equal to the distance.

Remarks.

Let the number of observations be n , and the angular distance D. The arch given by my improved circle will be $= D(n-1)$. In Borda's circle, (reducing the divisions of the sextant to those of the theodolite I use,) the arch is $= D \times \frac{1}{2}n$; and, either n must be an even number, or the odd observation must be lost. In Mayer's circle, the arch is $= \frac{1}{2}D \times \frac{1}{2}n$; and the number n , which comprehends the observations for the parallelism of the glasses and those for the distance, must likewise be even. The comparison of these expressions, shews at once the relative advantages of the different instruments.

My construction offers considerable advantages, in every manner of using the circle. If, instead of the crossed observations, it should be wanted to employ the usual practice of rendering the glasses parallel, a multiple of the distance may still be obtained by my instrument, equal to that of the other method. For this purpose, the parallelism of the glasses may be effected, by means of the images of the sun, or the horizon of the sea, moving the index F, while the 0 of the nonius is adapted to 360° of the limb, and the two clamps A, B, are fastened. After this, an observation of the distance to the right may be made, with the clamp A fastened, while the clamp B is loose; the clamp D being also fastened, and the clamp C loose; and, at the end of this observation, the flying nonius will give an angle, which will be only the half of the distance in my divisions, but which would be equal to the whole distance, if the divisions were according to the sextant. After that, and while the clamps B and C are fastened, and the clamps A and D loose, the parallelism of the glasses may be

be again effected; and the nonius will advance the same quantity over the limb. The same addition will take place, by inverting the use of the clamps, and making another observation of the distance. The like alternate process may be continued indefinitely; and the result given by the instrument will, with only one observation more, be the same as that of Borda's method, and double the arch which would be obtained by Mayer's circle.

Mr. Borda's circle is liable to a very great inconvenience in practice. Each index advances successively over the limb; and, in order to facilitate the operation of bringing the images for the contact within the telescope, that author advises to make a preparatory memorandum of the positions which the indexes will nearly occupy, so that they may be set accordingly, previous to each observation. But this method, which is always inconvenient, by night becomes almost impossible. For this reason, I have joined to the horizon index an arch L L, (Plates XXX. and XXXI.) which is divided, both to the right and left, into degrees and minutes of the sextant, so that, when the glasses are parallel, the centre index coincides with the two first divisions o, o, and occupies the space left blank between them. I have further provided two sliding pieces P, P, which may be adapted to that arch, with a spring sufficient to keep them firm in any situation. Putting each of these pieces upon the arch, so that their ends may coincide with the divisions marking the rough distance to be measured, no more will be required, than to set the centre index alternately against each piece, before the beginning of the successive crossed observation. The clamp may then be fastened, and the remainder of the motion produced by the adjusting screw; as, if necessary, the index will push the sliding piece further, and leave it at the point where the contact was effected*.

The flying circle facilitates the use of any number of noniuses, which may be applied round the whole circumference; but, as the leading principle which I have chiefly had in contemplation, is that of obtaining an accurate result from one reading, I have only used a single nonius. Two noniuses,

Easy contrivance for setting to the distance nearly, before each observation.

Other advantages, such as the increased number of noniuses, if required.

* The idea of this simple contrivance, was suggested to me by the ingenious Mr. E. Troughton.

opposite one another, might however be advantageous, in order to correct the errors of eccentricity; but, in my opinion, a greater number ought not, in any case, to be used.

It may be used in circles where the telescope is attached.

Before I conclude this Paper, I shall remark, that my improvement may be partially applied to a circle, where the telescope and the horizon glass are attached, or fixed, to the main frame of the instrument. The flying nonius, acting then with the single centre index, will only give the same result as Borda's circle; but this construction seems to me greatly preferable to all the other plans executed till now; the whole apparatus being more solid and simple, and its use not liable to the errors which arise from the motion of the horizon index.

It affords a method of repeating observations beginning from a stop.

With this construction, we may likewise employ a method of ascertaining the place where the parallelism of the glasses was observed to take place, and of setting the index afterwards in the same situation, as often as is necessary for the repetition of the observations. A piece may be used, so contrived as to be attached to, or detached from, one side of the index, by means of a screw; and provided besides with other screws, to fasten it to any part of the limb. This rectification piece, being previously attached to, and carried with, the index, must be fixed in the situation it occupied when the contact of the images was observed. The index will then be detached from it, in order to observe the distance, and afterwards must be brought back to the same position as before, contiguous to the rectification piece. The like alternate process may be repeated; and the flying nonius, going with the index in the motions forwards, and standing still in the motions backwards, will give the multiple of the observed angle, without performing the observation for the parallelism more than once in the beginning. In Mayer's circle, as well as in Borda's, there is a great objection to any attempt for that purpose; because, as the horizon glass moves round with the index, its perpendicular position is deranged by the inequalities of the plane of the limb; but, in my construction for multiplying with the horizon glass fixed, that inconvenience is removed; and the method of ascertaining the identical position of the glasses may be employed in practice, with advantage, it being done when the index is at the same point of the frame. By suggesting this idea, I do not, however, mean to represent it

it as preferable to the repetition of the observations ; which process must, for many reasons, have the advantage over any mechanical contrivance whatsoever.

I have procured reflecting circles to be constructed, upon the principles here described, both with the telescope and the horizon glass upon a moveable index, and with the same pieces attached to the main frame of the instrument. The two methods have respectively answered my expectation ; and I purpose, at a future opportunity, to publish a description of the means which I wish to recommend for the mechanical improvement of the different parts, together with an account of some other essays which I have made relative to the same subject.

III.

*Observations tending to investigate the Nature of the Sun, in order to find the Causes or Symptoms of its variable Emission of Light and Heat ; with Remarks on the Use that may possibly be drawn from Solar Observations. By WILLIAM HERSCHEL, LL. D. F. R. S. **

ON a former occasion this eminent astronomer gave his reasons for inferring that the sun is a most magnificent habitable globe. In the present paper he proceeds considerably farther in the investigation of the physical and planetary construction of that luminary. He introduces his subject by remarking, that the influence of this luminous body on the globe we inhabit is so great and so widely diffused, that it becomes almost a duty for us to study the operations which are carried on at its surface ; that from motives arising from the important effects of light and heat we ought certainly to examine their source, and though, like the Egyptians with the Nilometer, or modern philosophers in their observations upon the atmospheric phenomena, we can entertain no hope of modifying the effective causes, yet, by arranging the particulars of our own conduct by the fore-knowledge of events though inevitable, we may add much to the general felicity and enjoyments of society.

* Abridged from the Philosophical Transactions, 1801, p. 265.

W. N.

Dr.

New terms of
solar phenomena.

Dr. Herschel, in consequence of the improvements in his telescopes, and the advances in his knowledge of the physical construction of the sun, has rejected the old terms of *spots*, *nuclei*, *penumbra* & *luculi*, and has substituted those of openings, shallows, ridges, nodules, corrugations, indentations, and pores.

The luminous
matter of the
sun is of the na-
ture of clouds,
or atmospherical
phenomena.

In his former paper on the nature and construction of the sun and fixed stars, the author shewed that the lucid substance of the sun is not a liquid nor an elastic fluid, but that it exists in the manner of luminous clouds swimming in the transparent atmosphere of the sun, or rather of lucid decompositions taking place within that atmosphere. * His present observations confirm and extend that induction. They are arranged under certain enunciations, which I shall proceed to give, with so much of the particular facts as may be requisite for their elucidation.

Of Openings.

Openings; or
places where the
luminous solar
clouds are re-
moved.

Particular de-
scription.

Openings are places where the luminous clouds of the sun are removed, so as to exhibit the opaque globe of the sun through the aperture. On the 4th of January, 1801, there was a large opening much past the centre of the sun, with a shallow about it, as exhibited in Fig. 1, Plate II. On the preceding side the thickness of the shallow was visible from its surface downwards, but on the following side the edge of the shallow only could be seen, but not its thickness. The side of the elevation surrounding the shallow was also seen going curvedly down to the surface of the shallow on its preceding side. Fig. 2 represents a section of the same opening, in which the lines *a b c d f* are supposed to be drawn from the eye of the observer, corresponding with lines marked by the same letters in Fig. 1. The line *d* goes through the opening to the surface of the sun A B. Fig. 2 shews evidently why the thickness of the shallow and elevation of the luminous parts are seen on one side and not on the other.

General facts re-
specting the solar
openings.

Large openings have generally shallows about them. Many openings are without shallows. Small openings are generally without shallows. Openings have generally ridges and nodules about them. The openings have a tendency to run into each

* Philof. Transf. 1795, p. 72.

other,

other, and new ones break out near those already formed. The probable cause of openings is, that an elastic, but not luminous gas, issues up through the pores or incipient openings, and spreads itself on the luminous clouds, forcing them out of the way, and widening its passage. The direction and operation of this cause appears to be not equally extended on all sides, but is frequently oblique, so that the luminous clouds are driven, and form a larger shallow on one side. Different openings being effected similarly, seem to shew a correspondent effect or unity of direction to some extent.

Fig. 3 represents an opening, with a branch coming from its shallow, seen February 18, 1801. Fig. 4 represents the same opening as it appeared one hour afterwards, broken out at three places on the one side, with corresponding projections in its shallow, which is very large on the side towards which those breakings out point, but is more confined on the other quiescent side.

Fig. 5 represents a small oblong opening, with a very long shallow, the luminous matter being close to the opening on the other sides. Eight other small openings, forming a cluster, had each their shallows on the same side. In three hours it changed to the appearance at Fig. 6, and an hour afterwards an opening began to appear in the furthest end of the shadow, as at Fig. 7. When the shallows begin to diminish, and the lips or projections to disappear, the openings were observed to be at the period of their greatest extent. Fig. 8. There is some difference in the colour of openings, apparently from a thin veil of luminous clouds hovering above them. When the openings are decaying, they divide as in Fig. 9, in which observation, December 27, 1799, the luminous passage across the opening resembles a bridge thrown over a cave or hollow space. The opening was completely gone the following day. Openings sometimes increase after beginning to diminish. They generally grow less and vanish. They sometimes become converted into large indentations, with or without pores or small openings, and when they have vanished, they leave the surface disturbed more than common.

The depth of the openings is visibly apparent, and the distance between the shallows and the solar surface is indicated by the free motion of low clouds. Fig. 10 shows a large

They are produced by a wind or gas from the sun's body.

Changes in the openings observed.

Observations.

Observation of the depth, &c. of openings. Low moving clouds.

Narrative.

large opening observed January 25, 1801, at 9^h 22^m. The note is as follows: "A large opening, which I have been observing since the 19th, is now much advanced towards the limb. I can see into it; and on the preceding side, as it appears to me, a good way under the lowest regions of the clouds of which the shallow consists. The upper margin of the shallow is very sharply determined; but the clouds at the lower part of it, on the contrary, are more dispersed; some of them hanging a good way down towards the surface of the sun's body. See Fig. 11. At 10^h 20^m the preceding side of the shallow of the large opening is now more abruptly terminated at the bottom of its thickness; the hanging or projecting clouds being removed towards the following side. See Fig. 12."

Of Shallows.

Shallows, or places depressed below the luminous clouds.

Shallows are depressed below the general surface of the sun, and are places where the luminous solar clouds of the upper regions are removed. Their thickness is visible. They sometimes exist without openings in them. They begin from the openings or branch out from shallows already formed, and go forwards. The probable cause of shallows is deduced from the observations, among which the following are delineated.

Observations of the shallows;

January 25, 1801, 9^h 20^m. Two branches, A B, Fig. 13, of a shallow coming from an opening C, going towards the south. It seems as if they were destined to meet the incipient shallow of a south-following opening D. 9^h 50^m the shallow B is very nearly united to the narrow part of the shallow surrounding the opening D. The shallow H seems to advance in a direction towards the farthest south-following opening E. 10^h 20^m the shallow B is completely run into the shallow about D; and the shallow A is grown broader towards F. 11^h 30^m the shallow B is so completely joined to the shallow D, that it appears as if it had not come from the opening C. The shallow A now ends in a sharp point, Fig. 15. 12^h 50^m the shape of the shallow A is no longer pointed, but very broad at the end, and there is a new branch breaking out at G, Fig. 16. These changes seem all to denote that the shallows are occasioned by something coming out of the openings, which, by its propelling motion, drives away the luminous

they are caused by the propelling gas which produces the opening.

luminous clouds from the place where it meets with the least resistance; or which by its nature dissolves them as it comes up to them. If it be an elastic gas, its levity must be such as to make it ascend through the inferior region of the solar clouds, and diffuse itself among the superior luminous matter. 1^h 10^m the new branch G increases, and the openings C D E are enlarged. A new branch is also breaking out from the shallow about E. It is marked H in Fig. 14, and denoted with points. These changes seem to prove that the same gas which diffuses itself over the shallows, has forced open the passages at first, and is now widening them. Hence the increase of the openings is an additional circumstance which points out the cause of the shallows. 1^h 20^m: from the shallow of a very large preceding opening, which is in an increasing state, are lately projected three small branches, *a b c*, Fig. 17. 2^h 30^m the vacancies between the three small projecting shallows are now filled up by the same cause that occasioned them, so as to have given them the shape of an uniform but broader shallow on the side where the points came out.

The shallows have none of the appearance called corrugations, but are tufted. The close connection of the tufts makes them appear as if in every vacancy there were clouds under clouds, that prevented our looking far into them. The decay of the shallows appears to be produced by the encroachment of the luminous clouds on all sides, in consequence of the diminution of the energy which caused them.

Of Ridges.

Ridges are elevations above the general surface of the luminous clouds of the sun. The length of one of the longest was found to be about 75,000 miles. They generally surround the openings, though they are often seen in places where there are no openings. They are soon dispersed. From the appearances, the author thinks it probable that the luminous matter is disturbed at top by the transparent elastic fluid which issues from the openings, or that it may act below the luminous clouds, so as to lift them up or increase their mass.

Of Nodules.

Nodules, or
small elevations
of the luminous
matter.

Nodules are small, but highly elevated luminous places. They may be ridges four shortened.

Of Corrugations.

Corrugations are
smaller elevations
and depressions
of the luminous
matter.

The corrugations consist of elevations and depressions. They have a mottled appearance, consisting of dark and bright places. Many of the dark places are not round, but a little extended in different directions, and appear to be lower than the bright places. On favourable days the corrugated surface presents its elevations and depressions as distinctly as the rough surface of the moon. They extend over the whole surface of the sun. Dispersed ridges or nodules make corrugations. The corrugations change their shape and situation; they increase, diminish, divide, and vanish quickly. Fig. 19 exhibits a small sketch of the place of phenomena observed by the author and Dr. Wilson, in which the rapid changes were correspondently observed.

Of Indentations.

Indentations of
the luminous
matter.

The dark places of corrugations are indentations. That they are not much depressed, is deduced from their visibility pretty near the margin of the sun. The indentations go down at the sides like circular arches, but their bottoms are occasionally flat. See Fig. 19. Some indentations have no openings, others have. The corrugations are of all shapes, chiefly lengthened. The indentations are of the same nature as shallows; are of different sizes; sometimes containing very small openings, and sometimes changing to openings. They are extended over the whole surface of the sun, and with low magnifying powers they appear like points.

Of Pores.

Pores.

The low places of indentation are pores. They occasionally increase, and become openings: and frequently they vanish in a short time.

Of the Regions of Solar Clouds.

The shining
matter of the
sun cannot be a
liquid or a gas;

Dr. Herschel observes that the phenomena before described could not appear, if the shining matter of the sun were a liquid, because by the laws of hydrostatics the openings, shallows,

shallows, indentations, and pores would be filled up, and the ridges and the nodules would subside almost instantly. Whereas many openings have been known to last for a whole revolution of the sun, and extensive elevations have remained supported for several days. Still less could these phenomena consist with the supposition of elastic fluidity. It remains, ^{and consequently} therefore, says he, for us to admit this shining matter to exist ^{it must form} in the manner of empyreal luminous or phosphoric clouds, residing in the higher regions of the solar atmosphere. In support of this notion he gives the following observations.

Changes in the solar clouds happen continually. There are two different regions of solar clouds, the lower of which consists of clouds less bright than those which compose the upper stratum. The inferior clouds are opaque, the colour of all the shallows being the same, and consequently not affected by the cause which acts upon the upper stratum when shallows are generated. By a contrivance for the use of his photometer, he found that if the superior self-luminous clouds of the sun throw the same quantity of light on the inferior region of opaque solar clouds as they send to us, those inferior clouds, of which the shallows are composed, reflect about 469 rays out of 1000 they receive; but that the solid surface of the sun seen in the openings reflects no more than about 7. The indentations are planetary clouds, reflecting light through the open parts of the corrugations, and those opaque inferior clouds probably suffer but little of the light of the self-luminous superior clouds to strike the body of the sun. The motion of the inferior clouds is seen through the openings as they pass along. The superior clouds are also seen to transverse the same apertures previous to their closing.

That the planetary clouds are of eminent use, is inferred by recurring to the phenomena before recited. The planetary atmosphere of the sun, its great height, its density, as inferred from the power of gravitation, which is known to be twenty-seven times stronger at the sun's surface than with us, and must accordingly condense the atmospheric gases, its agitation similar to the winds of our planet, and the clear atmospheric space beneath the shallows, are among the facts upon which our philosopher extends his discussions. He repeats and amplifies his remarks respecting the operation of the gas, which is stated to pass from the sun itself upwards to the region of

Rapid sketch of the phenomena of the sun's surface.

Luminous clouds. Dark or planetary clouds.

Other facts and appearances.

Use and constitution of the solar clouds.

the clouds, so as to generate pores, corrugations, and all the solar changes, and most probably to maintain phenomena in the sun's atmosphere, which in ours would be mere transitory corrugations, like those of the aurora borealis, but are there, on account of the greater density, so compressed as to be-

Inquiry whether the energy of the solar heat and light be dependent on these phenomena?

come much more efficacious and permanent. If this account of the solar appearances shall be well founded, he concludes that there will be no difficulty in ascertaining the actual state of the sun with regard to its energy in giving light and heat; and that nothing will now remain but to decide the question which will naturally occur, whether there be actually any considerable difference in the quantity of light and heat emitted from the sun at different times. To ascertain this he has recourse to his journal.

Journal of solar observations.

In the first place he gives a set of observations, in which the signs of scarcity of luminous matter in the sun were apparent; namely, in general a deficiency of the luminous or empyreal clouds, no ridges, nodules, corrugations, or openings. This period lasted from the year 1795 to 1800. A second set, commencing in the year 1800, affords indications of a con-

Opinion that the seasons are governed by the variation in the sun.

trary nature. He expresses his opinion that the character of the seasons may be greatly dependent upon these phenomena.

Recurrence to Lalande for solar facts, and to Adam Smith for the fertility of the seasons.

But, in order to ascertain by some correspondent event or consequence that the seasons have been most productive when the solar spots were abundant, he recurs to the astronomy of Lalande for the solar appearances, and to the table in Adam Smith's *Wealth of Nations* for the prices of wheat during the same periods. The results are, upon the whole, favourable to his theory; though every cautious investigator into the laws of the universe must see that it is greatly in want of the support of observation in both respects. The Doctor, with his usual candour, states some of the difficulties of his subject, and concludes by adding, that his prediction of plentiful harvests from the present thriving state of the solar constitution ought not to be relied on with more confidence, than the facts and arguments he has presented may entitle it to. And from motives of the same kind, I have also taken the liberty to condense this part of his paper rather more than the other.

IN a subsequent paper of the same volume, Dr. Herschel gives a number of additional observations, made in the spring of 1801, by which it appears that the sun continues to exhibit symptoms of a plentiful emission of light. He strongly suspects that one half of our sun is less luminous than the other, and that its variable lustre may possibly appear to other solar systems as irregular periodical stars are seen by us; but whether this arises from some permanent construction of the solar surface, or be merely an accidental circumstance, must be left to future investigation.

He takes notice that we need not in future be at a loss with regard to the current temperature of this climate, as the thermometrical observations, regularly given in the Philosophical Transactions, afford a proper standard of comparison with the solar phenomena.

In this paper also, with a drawing, is described a method of substituting liquids instead of dark glasses, for moderating the solar light and heat in telescopes. It is a square trough, closed at two opposite ends by well polished parallel plane glasses. On one side of this trough is a small handle, and on the other a lip or spout for pouring out any portion of the liquid when the rest is to be diluted. The trough is made to fit into an excavation in the eye tube of the telescope, and in this situation the solar rays must pass through the liquid therein contained before they arrive at the eye. By colouring the fluid, the observer has it in his power to soften the light at pleasure; and water keeps off the heat so as to prevent the least inconvenience. It is easy to make trial of a variety of fluids in this way. Among other mixtures, the Doctor found that ink, diluted with water, and filtered through paper, shewed the solar image as white as snow, and very distinct. Through this mixture he could observe the sun in the meridian for any length of time without danger to the eye, or to the glasses, with a mirror of nine inches in diameter, and the eye-pieces open, as they are used for night observations.

IV.

Observations and Experiments upon Dr. James's Powder, with a Method of preparing, in the humid Way, a similar Substance.
By RICHARD CHENEVIX, Esq. F.R.S. M.R.I.A.*

Preliminary remarks.

AFTER the observations and experiments made by Dr. Pearson, to investigate the nature of Dr. James's powder, and presented by him to this Society, very little remained to be effected or desired, towards a further knowledge of the subject. But those very experiments served to suggest, that the mode of preparation was far from being the best that the present improved state of chymical knowledge might afford; and that, in all probability, a less defective composition might result from a process more conformable to some improvements, which of late have been advantageously applied to pharmaceutical chymistry.

It may be laid down as a general principle, that, in delicate experiments, whether analytical or synthetical, fire (that potent and once believed to be universal agent) is too precarious in its means, and too uncertain in its application, to be employed with full and constant success. And, if it is still resorted to, the advantage of promptness, and a remnant of ancient custom, are the principal reasons. But, where other methods can be devised to effect the same combinations, (and the humid way offers many,) every person conversant in chymical knowledge will allow the benefit of adopting them. The recent improvement in the mode of preparing calomel, is a striking example of such salutary corrections being successfully introduced.

The method of preparing James's powder, or Pulvis Antimonialis.

A few observations upon the formula according to which Dr. James's powder, or the *Pulvis Antimonialis*, is prepared, and upon some properties of antimony, will place this assertion in a more prominent point of view.

In order to prepare this powder, we are told to take equal weights of bone or hartshorn shavings and crude antimony, and calcine them together, at a high temperature: in other words, to take phosphate of lime, which already contains a great excess of lime, and add to it an oxide of antimony. In

this process, it has been supposed, that the phosphoric acid of the bone or hartshorn will saturate, not only the lime with which it was originally combined, but, in addition to it, a new portion of metallic oxide, and a new portion of lime. For, what little sulphuric acid might, during the process, have been formed by the combustion of the sulphur of the crude antimony, is dissipated, at a much lower temperature than that to which the powder is exposed.

Every oxide of antimony with which we are acquainted, is volatile at a high degree of heat: it would therefore be hazardous to assert, that it is possible to preserve always the same proportion of antimony, whatever care may be employed in directing the operation; and, a dissimilarity in the chemical result, must necessarily be attended with uncertainty in the medical application.

To this property may be added another, no less conducive to error. That portion of oxide of antimony which is not volatilized, becomes, in a great measure, insoluble in all the acids. What the effect of the gastric juice may be, upon a substance which resists the action even of nitro-muriatic acid, it is not my purpose to determine. It is sufficient for me to say, that, as the quantity of insoluble matter, in a given quantity of Dr. James's powder, prepared at different times, may vary, the effect of any dose also may differ, according to the proportions of soluble and insoluble matter.

I look upon it as a fortunate circumstance, that those experiments and observations which I mentioned in the beginning of this paper, existed as a standard to which I might refer my own attempts, and by which I might estimate their validity. Dr. Pearson has proved, (as by my own experiments I have found,) that in Dr. James's powder about 28 per cent. resisted the action of every acid. In examining some of the *Pulvis Antimonialis* of the London Pharmacopeia, I found the average quantity of insoluble matter to be about 44 per cent. This proportion, however, was liable to considerable variation.*

* I find, from the information of several medical gentlemen, that the *Pulvis Antimonialis* is generally considered as stronger than Dr. James's Powder. This seems rather extraordinary, when we consider that the quantity of insoluble matter is greater in the former than in the latter; and would almost lead us to suspect it to be the active part of the medicine.

The

Whether this powder be a ternary compound.

The powder here treated of is denominated, by Dr. Pearson, a triple salt, or a real ternary combination of a double basis, (lime and antimony,) with phosphoric acid. What I have mentioned, with regard to the quantity of acid contained in bone or hartshorn, as being too small to saturate a new portion of these bases, may throw some doubts upon the possibility of any such combination in the present case. But I have made some more direct experiments, which tend to prove, that no such combination does exist.

It contains no phosphate;

I took some white oxide of antimony, (formerly called Algaroth Powder,) precipitated by water from muriate of antimony, and heated it for a long time with phosphoric acid. I decanted the liquor, and washed the powder that remained. No antimony could be found in the liquor; nor could any traces of phosphoric acid be detected in the residuary oxide of antimony. I then took a solution of muriate of antimony, and divided it into two equal parts: into one, I poured distilled water; and, into the other, a solution of phosphate of soda. In each liquor, a copious precipitate was formed; which precipitates, after being well washed, were dried. The weight of both was the same; whereas, it is evident that, had any phosphoric acid been combined with the oxide, there would have been an augmentation of weight, in that which was precipitated by the solution of phosphate of soda. This precipitate likewise, upon examination, gave no traces of phosphoric acid. From these experiments it appears, that there exists no combination, which can be denominated a phosphate of antimony.

neither does it appear to be an antimoniate of lime.

To attempt an explanation of the real nature of the powder here spoken of, I had recourse to some experiments of Mons. Berthollet. By detonating sulphuret of antimony and nitrate of potash, in a crucible, he obtained a mass, which he reduced to powder, and washed. The liquor gave, upon evaporation, a crystallized salt, which M. Berthollet terms an *antimoniate of potash*. I never could succeed in any attempt to form a similar combination between the above white oxide of antimony and potash, owing, I believe, to the small quantity of oxygen contained therein, compared with that which is combined with the oxide obtained by detonation. I cannot therefore say, that the powder in question is, in any degree, what M. Berthollet would call an *antimoniate of lime*.

But,

But, be the state, whether of mixture or of combination, what it may, my purpose is to endeavour to produce a substance, which, from its more certain mode of preparation, may be more equal and constant in its effects.

Dissolve, together or separately, in the least possible portion of muriatic acid, equal parts of the forementioned white oxide of antimony and of phosphate of lime.* Pour this solution gradually into distilled water, previously alkalized by a sufficient quantity of ammonia. A white and abundant precipitate will take place, which, well washed and dried, is the substitute I propose for Dr. James's powder.

New process for a combination of antimony of more constant effect.

The theory of this precipitation is so clear and simple, that it does not require any comment. It may be useful, however, to those who wish to make this preparation, to remark, that it is absolutely necessary that the solution of phosphate of lime and of oxide of antimony, in muriatic acid, should, after being well mixed, be poured *into the alkaline liquor*, in order to obtain a precipitate homogeneous throughout the operation. For, should the alkaline liquor be poured *into the acid solution*, the water of the former would act upon the entire mass of oxide of antimony, while the alkali would precipitate the phosphate of lime only as it saturated the acid which held that salt in solution: thus, the precipitate would contain more antimony in the beginning; and, towards the end, the phosphate of lime would be predominant. For the same reason too, a pure alkali is preferable to its carbonate; for the carbonic acid disengaged, would retain in solution a portion of phosphate of lime.

Remarks.

* In order to procure the phosphate of lime, I dissolved in muriatic acid, a quantity of calcined bone, and precipitated by ammonia, in its state of greatest causticity. By this means, the excess of muriatic acid, which held in solution the phosphate of lime, is saturated, and the phosphate is precipitated; but no muriate of lime is decomposed, if the ammonia is quite free from carbonic acid. This is the most direct method of obtaining phosphate of lime pure. This salt is not decomposed, as some have asserted, by muriatic acid, but merely dissolved by it. I have been induced to state fully these particulars, because, from the beneficial effects of this salt in the treatment of rachitis, as proposed by M. Bonhomme, (*Annales de Chimie*, Vol. XVIII. p. 113,) it may become of general use. The oxide of antimony, I obtained by precipitating, by water, the common butter of antimony of the shops.

It is probably a mixture.

Whether this composition be a chymical combination or a mixture, I will not take upon me to determine; but, for the reasons above mentioned, in speaking of Dr. James's powder, I believe it to be merely a very intimate mixture. At all events, it must be more homogeneous than any that can be prepared in the dry way. It is entirely soluble in every acid that can dissolve either phosphate of lime or oxide of antimony separately; and, to have it constantly and uniformly the same, no further address, in preparing it, is required, than to avoid the errors I have mentioned.

Stronger preparation.

As, after some medical trials of the powder, it was suggested to me, that it might be advantageous to render it somewhat stronger, I prepared another portion, by taking two parts of oxide of antimony and but one of phosphate of lime, and precipitating as above described. The medicinal power was then considerably increased.

Medical testimony of its use.

Dr. James's powder is a medicine which has been so long in use, and is so deservedly ranked among the most valuable we possess, that every attempt to render the process for preparing it more simple and more certain, must be allowed to be of some importance. But, whatever reason there was to think, by arguing upon its chymical properties, that I had really succeeded in improving its medicinal virtues, it still remained to be proved, by actual experiment, that the hoped-for success was not merely conjectural. To ascertain this, I gave some of my powder to Dr. Crichton, Dr. Babington, and Mr. Abernethy; gentlemen whose extensive practice and acknowledged skill sufficiently enabled them to judge of its medical properties. They all concur in opinion, that, in its general effects, it agrees with Dr. James's powder and the *Pulvis Antimonialis*; but, that it is more mild, and consequently may be given in larger quantities, seldom producing nausea or vomiting, in doses of less than eight or ten grains.

V.

Construction of an Hydraulic Apparatus, which by Means of the Syphon raises Water above its level, and performs its Alterations without Attendance. By Mr. Wm. CLOSE.

To Mr. NICHOLSON.

SIR,

Dalton, Nov. 6, 1801.

AT the time when I wrote to you on the application of the syphon to raise water above the reservoir, I could find no easy method of making the instrument afford an equable supply without attendance. Since then, however, I have thought of a combination for the purpose, applicable to any height under an elevation of thirty feet; and another to raise water below the reservoir, independant of the pressure of the atmosphere.

New method of raising water by the syphon.

I send you a drawing, and description of each.

Plate III. exhibits a representation of the apparatus for raising water above the reservoir. An air pipe is inserted into one side of the turn of the syphon SS, and a small plate is fixed on the side of its aperture opposite the shorter branch to obstruct part of the current. At some distance from the syphon this pipe divides into two branches, 2, 3, which lead to two vessels A and B. At the separation is fixed a cock of such a construction, that the air pipe 1 can only communicate with one of its branches at once. The barrel of the cock, or that part which is represented by short radial lines in the section of this part of the apparatus, has three lateral openings: the plug or key which moves within the barrel contains a duct or channel, which, instead of passing straight through it, is turned with such a bend, that when one end of it is under the opening of the air pipe, the other will be opposite the orifice of one of the pipes 2, 3. The communication from the air pipe to either of its branches is therefore completed by about a quarter of a turn.

Drawing and description of apparatus.

The vessels A and B have each a pipe with a valve at its lower end, connected with the cistern CC, to carry up water, and two other pipes, one to let out the raised water, and the other to take in air at the same time. The air pipe need only be

be

CONSTRUCTION OF AN HYDRAULIC APPARATUS.

be very small, but the water pipe must be wide enough to empty the vessel in the same space of time that it was filled. Each has a valve, but as they are all upon the same level, only the valves of the two water pipes are represented. All these valves are closed by weights. Between these vessels is represented a wheel, with a weight suspended on one side, and the vessel E on the other. Its rim has a groove, for the chains or cords, and one side is formed into teeth, to act upon another toothed vessel above, which moves the cock in its axis.

The vessel D receives water from the cistern C C, through an aperture of a proper width, and is emptied by a syphon as often as the water rises to a certain height.

The syphon S S is about six inches in diameter; its shorter branch rises thirty feet above the reservoir, and its longer branch reaches five or six feet below the surface of the water.

Its effects.

Suppose now that this instrument is set to work and supplied with a sufficient quantity of water. The cock in the air pipe 1 above the vessel A must be opened, and the vertical pipes A C, and B C, must be filled with water, by opening a very narrow passage between the air pipe and each of its branches, while the valves of the vessels are closed.

This mode of proceeding is adviseable to prevent the sudden pressure of the atmosphere from forcing the air out of the empty pipes, with such rapidity as would destroy the operation of the syphon. After this, if all the valves are in good order, and all parts of the apparatus complete, no further attention will be necessary.

The periphery of the lower wheel moves between the valves of the vessel A on one side, and those of B on the other, and has a claw or pin to depress each in due order. The weight suspended upon the lower wheel opens the valves of B, and by the assistance of both wheels opens a communication between the air pipe 1, and the vessel A, whose valves have previously closed their pipes. The pressure of the atmosphere accordingly fills A with water, and the air it contained is carried down the stream through the longer branch of the syphon. About this time the vessel D is filled to a level with the bend of its syphon which conveys water in the vessel E. This last vessel being filled, overbalances the weight, and moves the two wheels; the valves of the vessel B are closed by their respective weights, and the rarefying tube is made

to

to communicate with the branch 3 ; the aperture of the key duct, which before corresponded with the end of the branch 2, is now placed under the orifice of the rarefying tube, and the opening which was there before, is turned to the branch 3.

At the time when all communication between the vessel and syphon is interfectcd, one of the claws or pins on the farther side of the lower wheel, opens the valve of the air pipe in the vessel A, and immediately after it the valve of the pipe for letting out water is depressed by a claw on the nearer side. This last valve will make little resistance, as the whole pressure of the atmosphere was removed by opening the small pipe first ; for this purpose it has a separate valve.

The raised water flows out of A while B is filling.

When the vessels D and E are emptied, the large weight moves the wheels and the cock ; the valves of B are opened in the same order as those of A, which being again connected with the syphon, is filled while B is emptied.

In actual work the receptacle of raised water must be placed under the vessels, the axis of the lower wheel may extend across the receptacle, and have two arms or levers, fixed in a proper position to depress the valves, while the wheel itself will move on the outside. The key or plug of the cock turned by the axis of the higher wheel, should move very true in its barrel. The higher wheel should be made light if the key of the cock is fixed fast to its axis. The key or plug should be frequently taken out and rubbed with tallow ; its tightness may be adjusted by a screw fixed in a proper frame and moved by a weight. When any repair is requisite, the cock in the air pipe above A must be turned.

Remarks and
observations.

A syphon six inches in diameter will require its air pipe to be about one inch and a half in diameter.

By a syphon of these dimensions water may be raised to the height of sixty feet, if the vertical water pipe of the vessel B, be placed in the cistern which receives the raised water from A, and the branch of the air pipe 3 be carried to the top of the vessel B. Both the vessel and the air pipe should be made something less on account of the greater body of air to be rarefied. The air pipe should reach considerably above the vessel, or it must have a float valve, to prevent any waste of the raised water.

If

If the lower end of the pipe B C was inserted into the lower side of the vessel A, the water might be forced out of this vessel into the higher, by admitting the pressure of the atmosphere. The higher water pipe however, should descend considerably below the bottom of the vessel, and its valve should be always kept in good order, or part of the raised water will be drawn down again when the air in the lower vessel is rarefied. Indeed if we can insure the tightness of the valves, water may be carried through any number of vessels, without being delivered into any cistern before the requisite height is obtained.

Construction of
the apparatus for
raising water
below the refer-
voir.

The apparatus represented in Pl. III. may be used to raise water below the reservoir, if the vessels and their water pipes are sufficiently settled, and the air pipe 1 placed in a vertical position; but for this purpose the combination represented in Pl. IV. will be preferable. It is an inversion of the former method. A stream of water falls down the pipe L, passes through the vessel V, and ascends again through the shorter pipe S. When there is a sufficiency of water to fill the longer pipe, the stream flows through the apparatus with considerable celerity. The longer pipe L having several small holes at the turn near the higher end, a quantity of air is drawn into the stream, and descends into the vessel V, where it quits the current, rises to the higher part of the vessel, and depresses an equal bulk of water, which is carried through the pipe S. This explains the principle.

The air however is not suffered to collect in the vessel V, but is conveyed through the pipe 1, into one of the vessels A B, previously filled with water, which it forces into the stream. The alternate connection of 1, 2, and 1, 3, is completed by a cock of the same construction, as is described in the explanation of Pl. III. and by the same kind of machinery.

The lower parts of the apparatus, Pl. IV. to above the vessels A B, are immersed in water. At present the vessel V communicates with the vessel A; the water will accordingly descend out of A through the tube below 2, which has a valve opening downwards. A small conical valve at the lower end of a pipe inserted into the top of B, being opened immediately before the communication between V and A is completed, the condensed air rushes out of B, and water enters through an aperture under that letter, while the valve under 3 is closed.

When the vessel B is filled, and A is emptied, the cock in the air pipe begins to be put in motion, but before the tube 1 is connected to the branch above 3, the small conical valve above B closes its air pipe; the valve under B being now closed by the pressure of the water in the tubes L S, the water in B is forced down the pipe under 3, and carried away with the stream: the condensed air is let out of A, and this vessel replenished with water.

The air which is carried down the stream while it can enter into neither vessel above, will collect in V, until an avenue to one is completed, through which it will rapidly ascend. If the engineer does not like the construction of the cock I have recommended, its place may be supplied by two of the common kind, with adjusting pieces upon their keys.

The condensed air collected in the vessel V, may be applied to the purpose of raising water either above the reservoir C C, or below the vessel V, merely by forcing the air out of V into the vessel containing the water, by the pressure of the water in the pipes L S.

The apparatus will begin its operations without any trouble; the capacities of the vessels A B are indeterminate; and if the higher orifice of the pipe S be contracted, the machine will work with a less quantity of water than it may be calculated to receive. The vessels A and B however, should always be filled completely with water before the admission of the condensed air, otherwise the quantity of raised water will decrease.

A model was constructed to illustrate the principle: the two water pipes were about half an inch in diameter, and only one small hole was made at the higher end of the longer. Into the top of the vessel represented by V, was soldered two small pipes, which were surrounded by a little thread, and inserted into the neck of a small glass bottle. The vessel V and the lower parts of the water pipes were plunged into a vessel of water, until all the air in the apparatus below the surface was expelled. In this situation the bottle, previously filled with water, was fixed upon the pipes above V. The model being lifted out of the water, and the longer pipe fixed to a small cistern, the remaining parts of the water pipes were carefully filled; a sufficient supply was let into the cistern, and the higher

Experimental trial.

higher orifice of S was opened. When the stream had acquired its full force, bubbles of air entered the bottle, and the water was quickly expelled.

I am, SIR,

Your humble Servant,

WILLIAM CLOSE.

ERRATA in the Paper on Wind Instruments. (Philos. J. V. 213.)

Page 217 line 23, erase *major*.

— 219 — 44, for *fifth* read fourth.

— 221 — 11, change G, a, B, into G, A, B.

VI.

Outline of the Properties and Habitudes of the Metallic Substance, lately discovered by CHARLES HATCHETT, Esq. and by him denominated Columbium.

ON the 26th of November last a Paper was read at the Royal Society, announcing the discovery and investigation of the properties of what appears to be a new metal; and as by the course of publication it must be several months before it will appear in the Transactions, I shall present my readers with the following outline :

History of the mineral.

1. The mineral was sent with some iron ores to Sir Hans Sloane by Mr. Winthrop of Massachusetts, and there is therefore every reason to believe, that it came from some of the iron mines in that province.

External character.

2. It is heavy, and of a dark grey nearly black; it in some measure has the appearance of the Siberian chromate of iron.

Habitudes with acids.

3. The nitric, muriatic, and sulphuric acids act but very feebly upon the mineral; of these however, sulphuric acid produces the greatest effect, and dissolves some iron.

With potash by fusion, and subsequent digestion in muriatic acid.

4. When melted with five or six parts of carbonate of potash it is partially decomposed; but in order to effect a complete decomposition, the ore must be alternately melted with potash, and digested with muriatic acid, which takes up the iron.

5. During

5. During the fusion, carbonic acid is expelled, and the pot ash becomes partially neutralized by a metallic acid, which may be separated after solution in water, by nitric acid added to excess, and then appears in the form of a copious white flocculent precipitate. The potash holds a metallic acid; precipitable by nitric acid.

6. The ore consists of more than three fourths of this substance combined with iron. This acid very abundant

7. The white precipitate is insoluble in boiling nitric acid, and remains perfectly white. Insoluble in nitric;

8. Boiling muriatic acid dissolves it when recently separated from pot ash. But soluble in muriatic;

9. Sulphuric acid when strongly heated also dissolves it. And sulphuric acid.

10. The acid solutions when saturated by alkalis afford white flocculent precipitates; prussiate of pot ash forms an olive green ditto; tincture of galls, deep orange coloured ditto; water also when copiously added to the sulphuric solution precipitates this substance in the state of sulphate, which as it becomes dry, from white, changes to blue, and lastly to grey. Precipitable.

11. Zinc forms a white precipitate.

12. The white precipitate combines with pot ash and soda, both by the dry and humid ways; it expels carbonic acid, and with pot ash forms a glittering scaly salt, which has much the appearance of the boracic acid. Neutral salts and their affections.

13. Acids separate it from the fixed alkalis, and when added to excess do not dissolve it unless heated, but even then nitric acid has no effect.

14. The same is observed when alkalis are added in excess to the acid solutions.

15. Hydro-sulphuret of ammoniac added to the alkaline solutions forms a chocolate coloured precipitate.

16. Ammoniac will not combine with the white precipitate.

17. When prussiate of pot-ash, or tincture of galls, are added to the alkaline solutions, no effect is produced till an acid is added, and then the usual olive green and orange coloured precipitates are obtained.

18. The acid and alkaline solutions are colourless.

19. The white precipitate will not combine with sulphur in the dry way.

20. It forms a purplish blue glass with phosphate of ammonia.

21. It reddens litmus paper.

22. It appears to be of extreme difficult reduction.

Conclusion.

From the above properties it appears to be an acidifiable metal different from those already known, and it has been therefore distinguished by the name of Columbian.

VII.

On the Sebacic Acid, or Acid of Fat. By CIT. THENARD *.

Authors who have treated the subject.

A GREAT number of chemists have successively written on the sebacic acid. Grutmacher appears to have been the first who has spoken of it; it has since been treated of by Rhades, Knap, Segner, and Haller, who have confirmed and augmented his experiments. Crell has given us several processes for obtaining this acid in a state of purity, and has subjected it to a number of experiments, the results of which Citizen Guyton has inserted in the *Journal de Physique*, (Vol. XVIII. pages 110 and 383, and Vol. XIX. page 384.) Bergman has ascertained the order of the attraction of this acid for the salifiable bases. Citizen Guyton has handled the same subject, and discovered several errors which had escaped the notice of this chemist.

Object of proof by them differs considerably from the result of the present experiments.

The object of all the experiments which I have just mentioned, is to prove that the product of the distillation of fat contains a peculiar acid, of extreme volatility, and of such a poignant and suffocating nature, that it cannot be respired without some degree of danger. Those of which I am about to give an account prove 1. That this product actually contains a peculiar acid, which however, so far from being volatile, odorous and suffocating, is on the contrary solid and inodorous; 2. That it also contains acetous acid; 3. That this new acid has no part in the smell of distilled fat; 4. That by all the processes that have hitherto been employed for extracting the sebacic acid, nothing is obtained but a foreign acid, and that consequently the sebacic acid has not yet been known.

* Communicated to the National Institute of France, and inserted in the *Annales de Chimie*, XXXIX. 193.

A. Processes for obtaining the Sebacic Acid, and Characteristic Properties of this Acid.

HAVING distilled a considerable quantity of the fat of pork, I treated the product, at several times, with hot water, and poured acetite of lead into the liquor; a flaky precipitate was formed, which I collected and dried, after which I put it, together with sulphuric acid, into a retort and heated it. The liquid of the receiver had no character of acidity; but in the retort a melted substance floated at the top, analogous to fat, which I carefully separated, and after having washed it well, boiled it with water. By the aid of the action of the heat, the water totally dissolved it, and by refrigeration crystalline needles were deposited, having little consistence: these needles were acid, and possessed properties altogether peculiar to themselves. In order to assure myself that they were not the product of the sulphuric acid, I treated distilled fat with water; and having filtrated and evaporated the liquor, I obtained needles possessing precisely the same properties.

In order to procure this acid, we may also, if we think fit, after having treated the distilled fat with water, saturate the filtrated liquor with pot ash, evaporate it, and pour into it a solution of lead. A precipitate is formed, which is a sebate of lead, that is to be treated as above with the sulphuric acid. These are the three methods which may be employed in order to obtain the sebacic acid: its properties are the following:

It is without smell; its taste is slightly acid: it melts like a kind of fat; it considerably reddens the tincture of Tournesol; it is much more soluble in a hot than in a cold temperature. Boiling water, saturated with the sebacic acid, becomes solid by refrigeration; alcohol dissolves a large quantity of it; it crystallizes in small needles; by using proper precautions, we may obtain it in the form of long, large, and beautiful lamellæ, of a very brilliant appearance; it precipitates the acetite and the nitrate of lead, the nitrate of silver, the acetite, and the nitrate of mercury; it saturates the causticity of the alkalies, with which it forms soluble salts; with pot ash it produces a salt which does not attract moisture from the atmosphere, which has but little taste, and which, if we pour upon it sulphuric, nitric, or muriatic acid, becomes turbid, and deposits

Pork fat was distilled, washed with hot water, and acetite of lead poured in. The precipitate was dried and then distilled with sulphuric acid. No acid came over; but a matter resembling fat, floated in the retort. This being boiled with water proved soluble and crystallized by cooling. It is a peculiar acid; and may be obtained by washing distilled fat with water, without the use of sulphuric acid.

On the aqueous solution from distilled fat may be saturated with pot ash, then evaporated and decomposed by acetite of lead. The sebate of lead may then be made to yield its acid by the stronger attraction of the sulphuric.

Sebacic acid has no smell; is slightly sour; melts like fat; reddens turnsol; is more soluble in hot than cold water, and so much in the latter as to become solid by cooling. Alcohol plenti-

fully dissolves it. Crystallizable. Its peculiar attractions and habitudes.

sebacic acid; when its solution is concentrated and mixed with one of these acids, it becomes solid; finally it does not render turbid the waters of lime, barytes and strontites. These properties distinguish it from all the other acids, and evidently prove that it is an acid *suu generis*.

B. Means of separating the Acetous Acid from the Product of the distillation of Fat.

The aqueous solution from distilled fat contains acetous or sebacic acid, more largely the greater or less the heat of distillation. The former is separated by neutralizing with potash, and distilling with sulphuric acid.

THE product of the distilled fat is treated with water; the liquor is saturated with potash, and evaporated. When the substance is dry, it is introduced into a retort with dilute sulphuric acid, or phosphoric acid, and distilled: an acid is obtained, which has all the characters of acetous acid; with potash it forms a foliated salt. This salt is susceptible of being melted by means of heat; when exposed to the air, it deliquesces speedily and completely; its taste is extremely pungent; with a solution of nitrate of mercury, it forms a precipitate crystallized in the form of spangles. When sulphuric acid is poured upon it, acetous acid is disengaged in abundance. Sometimes the water with which the product of the distilled fat has been treated, contains hardly any thing else than acetous acid, so that in order to obtain acetite of potash, nothing more is necessary than to evaporate the liquid. The quantity of sebacic and of acetous acid formed in the distillation of fat, varies in proportion to the lesser or greater degree of heat.

C. Examination of the Odorous Matter, and of the distilled Fat.

The pungent odour of distilled fat is not produced by an acid.

I INTRODUCED fat, which had just been distilled, and the smell of which was extremely pungent, into a tubulated retort. I adapted to the neck of the retort, a receiver which contained tincture of Tournesol. Having distilled with a gentle heat, I thereby filled the receiver with a strong odour, and yet the colour of the tincture was not changed—a convincing proof that the smell of distilled fat is not owing to an acid; besides, if this smell depended upon an acid, it would probably disappear upon placing it in contact with the alkalis, as the acid would then be absorbed; but this does not happen. We must therefore conclude, that it depends upon a portion of the fat, converted into gas, and undoubtedly changed in its nature.

D. Examination

D. Examination of the Processes that have hitherto been employed for obtaining Sebacic Acid.

CRELL, in order to separate the sebacic acid from the product of distilled fat, first added to it a certain quantity of potash, after which he filtrated and evaporated it. As the salt which he obtained was mixed with oil, he calcined it, after which he re-dissolved it in water, and evaporated the solution. By this means he obtained a salt of considerable whiteness, and of a foliated texture; this he introduced into a retort, with sulphuric acid, and distilled it, whereby he obtained a pungent and fuming acid. But as this process appeared to him to be an inconvenient one, he had recourse to the following :

Persuaded that the acid of fat existed ready formed in the fat itself, and was not a product of distillation, he prepared a soap with fat and potash, and treated this soap with water, in order to dissolve the sebate of potash formed in it. But as the water, besides the sebate of potash, dissolved also a certain quantity of fat combined with potash, he added to the solution of this sebate of potash and of this soap, a sufficient quantity of alum. By this means he obtained sebate of potash, which had only an admixture of sulphate of potash; this he evaporated, poured sulphuric acid upon the dry substance, applied heat; and sebacic acid passed into the receiver.

In the chemistry of Dijon, we find a process different from that of Crell. According to this process, the fat is calcined with a certain quantity of lime, in a crucible; the substance is then lixiviated with a large quantity of water; the water which holds the calcareous sebate in solution, is evaporated; this calcareous sebate is introduced into a retort with sulphuric acid, and the sebacic acid passes into the receiver.

These three processes I have repeated with the utmost care, and have obtained the following results. The first of Crell's processes, and that of the chemistry of Dijon, afforded me an acid which has all the characteristics of the acetous acid: with potash it forms a foliated salt, which is deliquescent, has an extremely pungent taste, and on being treated with sulphuric acid, yields a large quantity of vinegar. If Crell, as he asserts, has obtained a fuming and pungent acid, I presume that it is a small quantity of sulphureous acid, proceeding from the decomposition

Crell's process by lixiviation of fat with potash, filtration, evaporation, calcination, and resolution of the salt; and lastly distillation with sulphuric acid.

Another by forming soap and treating with water, and some alum and distilling as before.

Dijon process. Calcination of fat with lime, lixiviation, evaporation, and distillation with sulphuric acid.

Crell's first process afforded acetous acid.

decomposition of a certain portion of sulphuric acid by the fat, or of the carbone of the acetous acid disengaged from its combination.

His second by mismanagement gave muriatic acid.

In following the second process described by Crell, we do not obtain any acetous acid, but an acid, which is nothing else but the muriatic. In fact, it forms with the nitrate of silver a precipitate insoluble in an excess of nitric acid; with soda it yields cubic crystals. If we pour sulphuric acid upon these crystals, a penetrating gas is disengaged, which, on being brought into contact with the air, gives rise to vapours: the same acid, mixed with nitric acid, dissolves gold. With the oxide of mercury it forms a volatile salt; with potash it forms a salt capable of being fused without undergoing decomposition. These circumstances render it probable that Crell has employed the potash of commerce, which always contains muriate of potash; for in repeating this process with pure potash, no acid is obtained, except an almost imperceptible quantity of vinegar. This vinegar is formed by treating the fat with potash and the sulphuric acid; for fat contains no acid, not even when it is rancid. At least, I have several times treated rancid fat with water, and have uniformly obtained a liquid, which did not redden the tincture of Tournesol.

But if well performed, vinegar.

E. Recapitulation.

Enumeration and comparison of the above results.

THESE experiments prove, in my opinion, what I have advanced in the beginning of this memoir; namely, that there exists in the product of distilled fat a peculiar acid, which, instead of being volatile, odorous, suffocating, is on the contrary solid, inodorous, and fixed to a certain degree; that, besides this acid, the product of distilled fat contains acetous acid; that the sebacic acid has no share in producing the smell of distilled fat, which probably depends upon some particles of fat, converted into vapour and altered in their nature. They prove, besides, that by the processes of Crell, and by that described in the chemistry of Dijon, we obtain only the muriatic or the acetous acid; that consequently the sebacic acid has hitherto remained unknown, and that in the present state of our knowledge it is a new acid.

Hog's lard only was used.

I ought to mention, that all the experiments which I have related have been made with hog's lard, and that I have not, like Crell, varied these experiments with human fat, the marrow

marrow of beef and tallow. I propose to repeat them with these different fatty substances, and think, even according to the experiments of Crell, that I shall obtain the same results. I have not as yet examined all the properties of the sebacio acid; but it is my intention to study them with care, and give an account of them to the class.

VIII.

Improvement in the Art of preparing Radical Vinegar. By
CIT. Y. PERES, Jun.*

THE experiment which enabled me to ascertain the real difference between the acetous and the acetic acid, and afterwards to propose to the learned a new theory of vegetable acidification in general, may be of great service in the art of preparing what is termed radical vinegar; or, to speak more accurately, it ought totally to change the process used for preparing this substance.

The process at present in use, consists in distilling the acetite of copper in large spherical vessels of stone ware. By taking the greatest care, a small portion is obtained of this *radical vinegar*, so much used in medicine and for domestic purposes. The difficulty of checking the expansion of this product, is an inconvenience which, being dependent on the nature of the process itself, has at all times been felt. Some improvements have been proposed; but as the nature of the operation was not understood, these improvements produced scarcely any advantage, and the process still remains very defective. It is easy to observe, that the violent heat which is used, is much more than sufficient for depriving the acetous acid of its carbon, and that it must reduce a portion of it to its first elements. I do not hesitate to assert, that at least half the radical vinegar is lost: The large quantity of gas that is obtained, incontrovertibly proves the truth of this assertion.

I have formerly shewn, by various deductions from earlier experiments, that *radical vinegar* is nothing but acetous acid deprived of carbon. I have shewn, that the ordinary process

Observations on
the distillation of
acid from acetite
of copper.

Radical vinegar
is deduced to be
acetous acid de-
prived of carbon.

* In a letter to the Editor of the Mag. Encyclop.

for preparing this acid effects nothing more than to abstract this excess of carbon; and I have completed my proofs, by preparing this acid myself, by a means which every one knows can have no other effect than to abstract carbon from the substances to which it is applied.

Simple process for making it. One part of sulphuric and two of vinegar are distilled.

I distilled a kilogramme of sulphuric acid with two kilogrammes of good white vinegar. I suddenly brought the mixture to ebullition, and obtained a very large quantity of radical vinegar, as white and pungent as that of commerce. This process is so simple and economical, that I thought it would be useful to extract it from the Memoirs which I have given upon this branch of chemistry, and offer it to manufacturers. I can assure them, that it will diminish the expences of the manufacture by three fourths. In fact, the sulphuric acid which remains may still serve for two more operations: but then it will be necessary to rectify the radical vinegar, for it will be found impregnated with sulphurous acid gas. It would be proper to try whether the action of manganese, which is used for ether, might not be applied to this rectification. I do not apprehend that this metal, in so high a degree of oxidation, is susceptible of being attacked by the acetic acid.

Manganese proposed.

Erroneous notion of the elective attractions of acetic acid.

An erroneous opinion obtains, with respect to the tendency of this acid to combination, which I think it incumbent upon me to refute. Chemists place it, in their tables of attractions, in a much higher rank than it will be found entitled to, upon an investigation of its properties with the least degree of accuracy. We find that it displaces only the carbonic, acetous and other weak acids. This error has arisen from the appearance of strength which it derives from the pungency of its smell. In this instance, however, the chemical properties of the substance are by no means proportionate to the impression it makes upon our senses. I shall add an observation which proves that this acid is much less powerful than is generally imagined; namely, that the vapour which it spontaneously emits, and which might seem to be its most acid portion, scarcely reddens paper tinged with tincture of tournefort. In fact it is nothing more than a modification of hydrogen, and it takes fire like ether.

Its fumes are scarcely acid, &c.

IX.

An Account of a new Eudiometer. By Mr. H. DAVY, Director of the Chemical Laboratory, and Lecturer on Chemistry to the Royal Institution.*

THE dependance of the health and existence of animals upon a peculiar state of the atmosphere, and the relations of this state to processes connected with the most essential wants of life, have given interest and importance to inquiries concerning the composition and properties of atmospheric air.

Composition of the atmosphere an interesting subject.

This elastic fluid has been long known to consist chiefly of oxygen and nitrogen, mingled together, or in a state of loose combination, and holding in solution water.

Oxygen and nitrogen.

A variety of processes have been instituted with the view of determining the relative proportions of the two gases, but most of them have involved sources of inaccuracy; and lately all, except two (the slow combustion of phosphorus, and the action of liquid sulphurets), have been generally abandoned.

The eudiometric processes commonly used are exceptionable.

Both phosphorus and solution of sulphuret of potash absorb the whole of the oxygen of atmospheric air at common temperatures, and they do not materially alter the volume, or the properties of the residual nitrogen; but their operation is extremely slow; and in many cases it is difficult to ascertain the period at which the experiment is completed.

I have lately employed as an eudiometrical substance the solution of green muriate, or sulphate, of iron, impregnated with nitrous gas; and I have found that it is in some respects superior to many of the bodies heretofore used, as it rapidly condenses oxygen without acting upon nitrogen; and requires for its application only a very simple and a very portable apparatus.

Solution of muriate, or sulphate, of iron, impregnated with nitrous gas, proposed to absorb oxygen.

This fluid is made by transmitting nitrous gas through green muriate, or sulphate, of iron, dissolved to saturation in water†. As the gas is absorbed, the solution becomes of a deep olive brown, and when the impregnation is completed it appears

How made.

* Journals of the Royal Inst. p. 45.

† Dr. Priestley first observed this process: for a particular account of it, see Researches, Chemical and Philosophical, p. 152. Johnson.

opaque and almost black. The process is apparently owing to a simple elective attraction; in no case is the gas decomposed; and under the exhausted receiver it assumes its elastic form, leaving the fluid with which it was combined unaltered in its properties.

Eudiometer tube.

The instruments necessary for ascertaining the composition of the atmosphere, by means of impregnated solutions, consist simply of a small graduated tube, having its capacity divided into one hundred parts, and greatest at the open end; and of a vessel for containing the fluid.

Manipulation.

The tube, after being filled with the air to be examined, is introduced into the solution; and, that the action may be more rapid, gently moved from a perpendicular towards a horizontal position. Under these circumstances the air is rapidly diminished; and, in consequence of the dark colour of the fluid, it is easy to discover the quantity of absorption. In a few minutes the experiment is completed, and the whole of the oxygen condensed by the nitrous gas in the solution in the form of nitrous acid.

Maximum of diminution.

In all eudiometrical processes with impregnated solutions, the period at which the diminution is at a stand must be accurately observed; for, shortly after this period, the volume of the residual gas begins to be a little increased, and, after some hours, it will often fill a space greater by several of the hundred parts on the scale of the tube, than that which it occupied at the maximum of absorption.

Subsequent increase; and why.

This circumstance depends upon the slow decomposition of the nitrous acid (formed during the experiment), by the green oxide of iron, and the consequent production of a small quantity of aeriform fluid (chiefly nitrous gas)*; which, having no affinity for the red muriate, or sulphate, of iron produced, is gradually evolved, and mingled with the residual nitrogen.

The muriate is preferable to the sulphate.

The impregnated solution with green muriate is more rapid in its operation than the solution with green sulphate. In cases

* The decomposition of nitrous acid, by solutions containing oxide of iron, as its minimum of oxidation, is a very complex process. The green oxide, during its conversion into red oxide, not only decomposes the acid, but likewise acts upon the water of the solution; and ammoniac is sometimes formed, and small portions of nitrous oxide and nitrogen evolved with the nitrous gas.

when

when these salts cannot be obtained in a state of absolute purity, the common or mixed sulphate of iron may be employed. One cubic inch of moderately strong impregnated solution is capable of absorbing five or six cubic inches of oxygen, in common processes; but the same quantity must never be employed for more than one experiment.

A number of comparative experiments, made on the constitution of the atmosphere at the Hotwells, Bristol, in July, August, and September, 1800, with phosphorus, sulphurets of alkalis, and impregnated solution, demonstrated the accuracy of the processes in which the last substance was properly employed. The diminutions given by the sulphurets were indeed always greater by a minute quantity than those produced by phosphorus and impregnated solutions; but the reason of this will be obvious to those who have studied the subject of eudiometry. In no instance was it found 100 parts in volume of air contained more than 21 of oxygen: and the variations connected with different winds, and different states of temperature, moisture, &c. were too small, and too often related to accidental circumstances, to be accurately noticed.

Comparative experiments, or trials.

In analysing the atmosphere in different places, by means of impregnated solutions, I have never been able to ascertain any notable difference in the proportions of its constituent parts. Air, collected on the sea at the mouth of the Severn, on October the 3d, 1800, which must have passed over much of the Atlantic, as the wind was blowing strong from the west, was found to contain 21 per cent. of oxygen in volume; and this was nearly the proportion in air sent from the coast of Guinea, to Dr. Beddoes, by two surgeons of Liverpool.

No remarkable difference observable in the air of various places as to its oxygen.

If we compare these results, with the results gained more than twenty years ago, by Mr. Cavendish, from experiments on the composition of atmospherical air, made at London and Kensington; considering, at the same time, the researches of Berthollet in Egypt and at Paris, and those of Marti in Spain, we shall find strong reasons for concluding, that the atmosphere, in all places exposed to the influence of the winds, contains very nearly the same proportions of oxygen and nitrogen: a circumstance of great importance; for, by teaching us that the different degrees of salubrity of air do not depend upon differences in the quantities of its principal constituent parts, it ought to induce us to institute researches concerning the dif-

—and consequently its relative salubrity depends on substances dissolved or suspended in it.

ferent

ferent substances capable of being dissolved or suspended in air, which are noxious to the human constitution: particularly as an accurate knowledge of their nature and properties would probably enable us, in a great measure, to guard against, or destroy, their baneful effects.

X.

Analysis of a newly discovered Mineral resembling the Hyacinth.

*By Professor THOMSDORFF.**

History.

THROUGH the kindness of Prince Gallitzin, I received a new mineral substance, denominated by him *red garnet* (rather *granat*), which greatly resembled the hyacinth in respect of colour, hardness, specific gravity, &c. The native country of this new fossil is Greenland. I shall call it *compact hyacinth* (*dichter hyacinth*.)

Action of heat.

In order to investigate the nature of this fossil, it was ignited intensely for two hours, and then suffered to cool. It suffered no loss of weight, nor was its beautiful red colour in the least impaired. It was submitted to a similar action of caloric six different times, but no change was effected. It was then reduced to a powder.

Powder boiled with potash, and then fused.

A. Two hundred grains of the impalpable powder were boiled for some time in a silver crucible, with a solution of potash, containing 500 grains of dry alkali. The fluid, after having been boiled for some time upon the fossil, acquired a yellow colour, and at last became green. It was evaporated to dryness, re-dissolved in distilled water, evaporated again, and lastly ignited at as high a temperature as the silver crucible would endure.

Mass, difficultly soluble in water, deposits a red powder.

B. The fused mass of the foregoing process, when cold, was of a dark green colour, inclining to a paler green on the sides which had been in contact with the crucible. Water seemed to have little action upon it, but after having been boiled in this fluid for a considerable time, it began to soften, and was at last dissolved. The clear solution obtained in this manner, after having been suffered to stand undisturbed, deposited a

* Translated from Crell's Chem. Annalen, 1801, Part VI. page 1.

reddish brown powder, greatly resembling the red sulphurated oxide of antimony. The supernatant fluid was perfectly transparent.

C. The whole fluid was then warmed a little, and muriatic acid added to it gradually. The powder before separated became thus soluble after this acid had been added in excess. It now acquired a dark green colour, and was perfectly transparent.

D. Having concentrated this fluid down to two ounces, no siliceous earth being deposited, it was evaporated to perfect dryness. The residue obtained was of a white colour. After being ignited, it weighed 110 grains. It was boiled again with pure potash in a silver crucible, evaporated to dryness, and fused. The fused mass was again dissolved in boiling water, muriatic acid was added in excess, and the whole evaporated to dryness. On adding some distilled water to the residue, an insoluble powder was obtained. This powder, after being washed in boiling distilled water, was dried, and then ignited. Its weight amounted to 100 grains. *It was pure siliceous earth.*

E. The fluid from which the foregoing product had been obtained, together with all the water expended in washing it, was collected together, evaporated, and mixed, whilst *boiling hot*, with a solution of muriate of potash. A reddish brown precipitate fell down, which, being collected upon a filtre, was washed in distilled water.

F. The precipitate, after having acquired a considerable degree of dryness on the filtre, was boiled during four hours in a concentrated solution of pure potash. The fluid was then attenuated with a considerable quantity of boiling distilled water, and transferred upon a filtre, of which the weight had been previously ascertained. The residue obtained, after a repeated ebullition with boiling distilled water, was suffered to dry, and then strongly ignited. Its weight was 32 grains. It was marked X.

G. The aqueous solution of potash, together with all the water used for washing the last obtained products, was concentrated by evaporation. On adding muriatic acid to it, a precipitate ensued; an excess of acid was therefore added, in order to re-dissolve it. The solution was then decomposed by means of carbonate of ammonia. The precipitate now ob-

Addition of muriatic acid completes the solution.

Evaporation to dryness, fusion with potash, solution and evaporation, as before, and the addition of water, separated much flux.

Precipitate, from last solution, by muriate of potash.

was dried, boiled in potash, then diluted, separated, and ignited.

The last solution was super-saturated with muriatic acid, and precipitated by carbonate of ammonia. It gave alumine.

tained was of a white colour. After being collected on a filtre, washed, and dried, its weight amounted to 36 grains. On dissolving it in sulphuric acid, and adding a minute quantity of potash, pure acidulous sulphate of alumine and potash was obtained. It was therefore pure *alumine*.

The precipitate X, ignited with potash and washed, was partly soluble in sulphuric acid.

H. The residue marked (X F) was now thoroughly dry, and of a considerable hardness. Its colour was a glittering reddish brown. It was reduced to powder, and digested in liquid ammonia. This alkali, however, after having being examined, had taken up nothing. Concentrated sulphuric acid being presented to a small quantity of it, had likewise no effect upon it. It was therefore mixed with a small quantity of dry potash, and ignited for some time; then dissolved in distilled water, and transferred upon a filtre. The residue was repeatedly washed. After being considerably dry, it was put into a glass evaporating basin, pure sulphuric acid was poured over it, diluted with a little water, and the whole carefully evaporated to dryness. On being suffered to cool, and distilled water being added, part of it became dissolved, and another part remained insoluble in the form of a brownish powder, which was collected on a filtre.

The solution contained iron.

I. This sulphuric solution (H) was of a greenish hue; its taste was astringent. Prussiate of potash threw down prussiate of iron. Tincture of galls produced a black precipitate with it. Liquid ammonia occasioned a reddish brown precipitate, which, after being washed, dried, and ignited, weighed twelve grains. It was an *oxide of iron*. It was assayed with super-saturated borate of soda by means of the blow-pipe, for investigating the presence of manganese, but no vestige of this metal could be discovered.

The sulphuric solution, H, gave (by carbonate of potash) a precipitate of zircon.

K. The product obtained by means of sulphuric acid (H), was mixed with three times its own weight of carbonate of potash, and boiled in a sufficient quantity of distilled water. A light earthy substance became separated, tinged slightly yellow, by an admixture of ferruginous matter. Being disposed to believe that this earthy substance might be zircon, it was merely collected on a filtre and washed, but not ignited. Its weight was twenty-five grains. It would, if it had been ignited, probably weighed only twenty grains. This earth was insoluble in potash; but soluble in nitric and acetic acid.

United

United with sulphuric acid to perfect saturation, it yielded a salt of a difficult solubility; but if this acid was added in excess, the salt was readily soluble in water. When ignited it became hard and insoluble in acids. I therefore do not hesitate to believe, that this earth was *zircon*, though not absolutely pure, but soiled with iron.

From these experiments it follows that 200 grains of this hyacinth-like fossil, consist of

100	grains	silica	(D)
56	—	alumina	(G)
12	—	oxide of iron	(I)
20	—	zircon	(K)
12	—	Loss of matter during the operation.	

200

XI.

Philosophical Disquisitions on the Processes of common Life.—Art of Shaving.—W. N.

THE caprice of fashion, or the modern improvements in Process of *shaving*, personal neatness, has deprived all the nations of Europe of *ing*. their beards; and consequently a portion by no means inconsiderable of the small and daily conveniences of life must depend on the facility with which this appendage can be taken off. This subject is frequently treated in conversation; and I am persuaded that many individuals will be glad to know what can be stated on the whole respecting it.

The fabrication of a good razor depends on so many circumstances and conditions, the material, the art of forging, *Instructions for chusing a good razor.* the hardening and the temper, that the artist himself after he has exercised his utmost skill; can in the last instance, select such instruments as are eminently superior to others, by trial or actual use. I am not aware of any means of chusing a good razor out of a number. All that I can say in this respect is, that a bad razor cannot be easily brought to a fine, or even a moderate edge; for which reason I should prefer that razor which possesses the best edge, and has been most slightly touched upon the hone: that is to say, the razor which, upon looking

Character of a
very fine edge.

Looking along its edge, has little or no flat part where the action of the hone has taken place, and which when drawn along the hand appears keen and smooth. I must here take notice, that the common practice of cutting the skin of the hand, in order to try a fine edge, is by no means so delicate as that of placing the edge of the razor lightly on the thick skin of the hand, so as to bear upon a length of about two inches, and then drawing it along through about one quarter of an inch, without cutting. In this manner the irregularities of edge in the finest surgeon's instruments may be plainly felt.

Art of giving a
fine edge to cut-
ting tools.

I will suppose the operator to be in possession of a good razor. He is in the next place to learn how to keep it in order. The original keenness of edge will necessarily go off by use. It can only be restored by whetting or grinding it up again. This is usually done by means of a good strap; in which little instruction is required besides what is common to every operation upon the edge of cutting instruments; namely, that they must never be sharpened but when actually dull, and that the operation of sharpening them is mischievous, if it be carried a moment beyond the time in which the full effect is produced. The act of strapping produces a smooth edge; but on account of the elasticity of the strap this edge becomes round and obtuse in the angle formed by its faces.

The strap.

Whetting upon
the hone.

A razor which has its edge too much rounded to be revived by the strap, must be sharpened upon the hone. We may consider a hone as composed of fine sand agglutinated together, the particles of which cut or rasp away the face of the tool that may be whetted upon it. This face will accordingly become filled with scratches, and the edge will be an irregular saw, with notches, so much the finer as the particles of sand were smaller, and the pressure of rubbing more light. It is found that the action of a hone is smoother, if it be left with oil upon the face when put by; an effect which probably arises from the oil becoming glutinous, and clogging the cutting points of the sand. The principal instructions for whetting a razor are, 1. That it should be drawn lightly along the stone by repeated alternate strokes with the edge foremost; and by no means backwards and forwards, unless a considerable part be required to be whetted away, which can seldom be the case unless there be a notch, or manifest irregularity in

Particular in-
structions.

in the edge; 2. That the edge should be tried upon the hand after every two or more strokes, in order to ascertain the instant at which it shall have become very uniformly rough. This roughness, if the hone be good, and the pressure light, will constitute a very fine edge, though in general less smooth than is left by a good strap; and 3. The edge will therefore be completed by a stroke or two upon such a surface. But I will suppose that a longer continuance of whetting has been necessary, so as to produce what is called a wire, consisting of a very thin film of steel adhering along the edge. If this should break off upon the hone during the whetting, the edge will become notched in passing over it; for which reason it must be taken off by a direct operation; namely, by passing the edge of the razor once along the hone, holding the back rather more upright than half way between the flat and the perpendicular position, and then passing it once back again, inclining the razor the contrary way. These two strokes will detach the wire, and produce a very perfect, though very obtuse edge; which must then be brought up to the requisite keenness by two or more light strokes in the first mentioned method, taking care not to go too far. In both the processes of strapping and whetting, we have supposed the razor to be laid flat; but where a strong edge is required, the whetting, but not the strapping, may be performed with the back of the razor elevated somewhat less above the face of the hone, than half the breadth of the blade.

Our cutting instrument being now in order, there remains but one more observation before we proceed to the subject of operation; which is, that it will be found to act considerably better after immersion in hot water. While I admit the fact, I must confess that I am far from being satisfied with any of the explanations. It has been said, that the expansion by heat enlarges the fine notches of the edge, and probably develops or opens others; but this effect can hardly be thought of any consequence, when it is considered that the whole expansion produced by boiling water does not exceed one part in ten thousand. Others have remarked, that as heat softens horn, hair, and other similar substances, the hot edge may pass through a hair more easily than if cold; but here we may remark, that the heat seems too little, and its application to the hair too momentary, to be productive of so considerable a

Wire upon the edge;

Taken off.

Enquiry how far heating a razor in water is advantageous.

By improving the edge;

Or by softening the hair;

**Or by cleaning
the blade.**

difference; besides which the advantage is said, and I believe truly, to follow even if the razor be suffered to cool before it is used. Hence it should seem as if the hot water detached some particles of grease from the edge, and by that means facilitated the sliding of the cut surface of the hair upon the face of the razor.

**Application and
utility of soap.**

There is much difference of opinion as to the application and use of soap. By some it is applied cold and thick by means of a brush; others apply it hot; and others again apply hot suds or soap-water, with much rubbing, until the alkali has rendered the skin much softer, and more disposed to be acted on by water than in its usual state. Sir John Chardin asserts, that the great excellence of the Persian barbers consists in this practice; but I can assert on the other hand, that the Chinese, who shave with exquisite facility, use a soap box and brush with cold water. Whether the effect of the soap be to soften the surface of the hair by an incipient combination of alkali; whether it renders the stroke more easy by causing the razor to slide with facility along the surface, instead of raising up portions of skin and cutting them off, as it might otherwise do, are questions to be solved by direct experiments. Of these I know none, excepting that a mere solution of alkali is less effectual than soap, and so likewise is the mere application of oil or fat. Hence probably we may infer, that soap acts in a twofold manner; by dissolving and removing the matter of perspiration by its alkali, and lubricating by its oil. With regard to the difference of heat and cold, those effects may perhaps be forwarded, and the hair somewhat softened at the higher temperature, though the difference seems to be not very considerable.

**Probable use of
soap.**

**Mechanical pro-
cesses or operation
of shaving.**

Some operators place the razor flat upon the face, and others raise it to a considerable angle. It is certain that the process may be skillfully performed either way. It is a very bad practice to press the razor at all against the face; and indeed this can not be done with impunity if a drawing stroke be used. Unskilful shavers will generally injure the skin less if they lay the razor flat; but generally speaking, the closest shaving will be performed by holding the razor at the same inclination as was used in whetting it.

**General Re-
marks.**

Upon the intire view of the subject it appears to me, that the only indispensable condition among the requisites before discussed,

discussed, is that the razor should be in good order: and that all the other particulars may be varied, except the mechanical process of cutting;—that the great secret of taking off the beard with facility, consists in a drawing stroke, that is to say, that the line of the motion of the razor itself should be very oblique to the line of the edge, and not at right angles to that line, as is commonly practised; so that the tool may be made to exert its action as a saw, which is much more powerful than its simple effect as a wedge. This method is indeed so very effectual, as to require great care and considerable practice before it can be adopted, in the extreme, with perfect safety; but the same efficacy which endangers the skin, renders it easy and pleasant with regard to the beard.

Essential part of manipulation.

XII.

Abstract of the Enquiries of CIT. BENEDICT-PREVOST, and some other Natural Philosophers, relative to the Motions of Odorous Substances placed upon Water. By CIT. BIOT.*

IT is a fact, which has long been known to natural philosophers, that small pieces of camphor, when placed upon water, have a very rapid rotatory motion. Volta and Brugnatelli have obtained the same results, by employing the benzoic and succinic acids. Citizen Benedict Prevost has extended this property to a great number of odorous substances.

Motions of camphor, &c. upon water.

But however unanimous philosophers have been in admitting the facts, they differ greatly from each other in the explanations they have given.

Citizen Prevost attributes these motions to the emanation of the odorous particles of the bodies: The experiments upon which he grounds this opinion may be found in the numbers that have been quoted. Venturi, Professor of Natural Philosophy at Modena, applies to these phenomena the explanation which Monge has given of the apparent attractions of substances floating upon the surface of water: according to him, "the water has a stronger attraction for the solid camphor,

Discussions respecting their cause.

Venturi supposes the action of water.

* Soc. Philom. 54. The experiments of Prevost, Venturi, and Carradori, alluded to in this notice, are given in the Philosophical Journal, 4to; for which see the Indexes. N.

than for the small portion of it which it has already dissolved and saturated; it ascends along the solid piece, and there forms a curvilinear inclined surface. The small portion that is dissolved and saturated descends along this surface, and, whilst it is descending, it pushes backwards, according to the laws of mechanics, the surface itself, and the solid piece that adheres to it." He thinks that we ought not to confound this effect with the repulsions which air impregnated with ether, or the exhalations of very hot camphor, exercises upon the light bodies which are made to float upon the surface of the water. In this case only he admits the presence of an elastic fluid. (*Annales de Chimie*, tome 21.)

Dr. Carradori, a
kind of oil.

Dr. Carradori is of a different opinion: He explains this motion by the elective affinity of a species of oil, which, according to him, proceeds from the camphor in contact with the water. He believes that the retreat of the water, which takes place upon a wet china plate or piece of glass, when camphor or other odorous substances are placed upon it, is the effect of the elective attraction of the surface of the plate or glass for the oil which the substances emit; and, according to him, it is this oil which causes the water to recede, by substituting itself in its place. (*Annales de Chimie*, tome 37.) In support of his opinion, Dr. Carradori alledges, that camphor does not move upon the surface of the water when this is very limited. He has not been able to make small plates of metal move, as Citizen Prevost had announced, by placing a morsel of camphor upon them, and letting them float upon the surface of the water. However, I have several times repeated this experiment, and always with success; but it requires a great deal of care and extreme accuracy.

New experi-
ments and obser-
vations of Pre-
vost.

Citizen Prevost has replied to Dr. Carradori, in a Memoir which he has addressed to the Society, and which is intitled, *Nouvelles Expériences sur les Mouvements spontanés de diverses Substances, à l'approche ou au contact les unes des autres*. The following are the principal facts which it contains:—

Ether acts at a
distance.

A drop of ether, placed upon a small plate of tin [*fer blanc*], 15 grammes ($3\frac{1}{4}$ drachms) in weight, throws it into a lively motion, though it does not touch the surface of this liquid.

Thus ether acts upon water at a distance. This fact may be verified in a very simple manner. If we place a small plate
of

of tin upon water, and bring the extremity of a glass tube wetted with ether within some centimetres distance of it, the plate recedes.

Small pieces of camphor, thrown upon mercury that had been well dried, were agitated there with the same motions as ^{Camphor upon dry mercury,} on the water. In order that this experiment may succeed, it is necessary that the mercury should be well cleaned or dried: the smallest particle of oil or fat spread upon its surface stops the motion. The pieces of camphor ought to be very small, the reason of which will be seen hereafter.

Very thin plates of mica, placed upon mercury, and charged with a small piece of camphor, move in the same manner as ^{Camphor upon mica floating on mercury,} upon water.

The benzoic acid likewise assumes a rotatory motion upon mercury; but it is necessary that it be reduced into almost imperceptible fragments. An oily aureola forms itself round these fragments. Nothing similar to this is observed round camphor, not even when examined with the microscope. The metallic lustre of the mercury is not impaired by it.

It results from these facts, that the presence of water is not ^{Water unnecessary,} necessary to the motions of odorous substances.

These substances cause the water to recede upon plates of alum, of pottery [*terre à faïence*], and of gum arabic, in the same manner as upon a wet plate of porcelain. This retreat is not therefore owing to the elective attraction of the oily or odorous substance for the surface of the plate.

Finally, notwithstanding the assertion of Dr. Carradori, camphor moves in very confined vessels: Citizen Prevost has seen it agitated in capillary tubes, into which it was introduced in very minute fragments.

From these facts Citizen Prevost concludes, that the intervention of an elastic fluid is necessary to the production of these phenomena. To the facts which he has adduced, I shall ^{Inference, that the motion is caused by an elastic fluid.} sub-join the following, which appear to me to decide the question relative to the motions of camphor upon water.

If we cut a small piece of camphor, of the weight of a few ^{Experimentum crucis.} grains, into the form of a cone, and approach it to the distance of four or five millemetres of a very small piece of leaf gold floating upon the surface of the water, presenting the point to it, this small piece of gold is repelled; and we may thus conduct it through the whole extent of the vase, without ever being

being able to touch it. It is necessary that the water should be very pure, and the vessel perfectly clean. We may hold the piece of camphor with a pair of tweezers, or at the end of a glass tube; it must be cut into a conical form, as has been mentioned; a piece of a larger size and an irregular form would envelope the light body in its atmosphere, and it would not move with equal facility.

The same effects are produced, if we employ, instead of camphor, a small piece of fine sponge soaked in camphorated water, or merely a tube of glass, containing at its extremity a drop of the same solution.

Variation.

If we cover a china plate with a very thin layer of water, and approach the piece of camphor of the preceding experiment within a few millemetres of it, presenting it by the point, in such a manner that the axis of the cone is perpendicular with the surface of the layer, the water recedes under the cone, and forms a circle concentric with it. The interior part of this circle is coloured with prismatic ranges, proceeding from the prolongation of the axis, and extending from the centre towards the circumference with a very rapid motion; after some moments, the circle loses its colour from the centre to the circumference, and the iris finally disappears, whether we continue to hold the camphor over the surface of the layer or not. It is indifferent whether we hold the plate in a horizontal or vertical position: the circle is always formed in a direction perpendicular to the axis of the small cone of camphor. I observed these phenomena at the temperature of 15° of Reaumur's thermometer.

Ether on water.

Lastly, if we throw a small piece of fine sponge soaked in ether upon water, it is instantaneously thrown into motion like camphor; and a hissing noise is heard, similar to that of water during its conversion into vapour upon a hot iron. If we view the surface of the water horizontally, before a light window, we observe sparkling jets issuing from the sponge, which extend themselves in serpentine windings upon the surface of the water, to the distance of some centimetres, and there produce irides similar to those of the preceding experiment. These irides soon disappear. During this emission, the sponge has a progressive and a rotatory motion, which are evidently owing to those small jets, the impulse of which it is observed constantly to obey.

Of these three experiments, the two first shew that camphor *Inferences.* acts upon water at a distance, and without contact; the third renders the manner in which its motions can be performed upon this liquid perceptible to the senses.

It appears to me, that from a comparative view of these facts, we may deduce the following inferences as certain:

Camphor is moved upon the surface of water by the effect of the emission of the particles which compose it; an emission that becomes perceptible to our senses by the smell which it produces, and by the repulsions which it exercises against small bodies floating upon the surface of the water.

As the effect resulting from these different impulses does not pass through the centre of gravity of the piece of camphor, this centre has a progressive motion, and the body revolves round it. As the figure of the piece of camphor changes every moment, the motion of its centre of gravity is neither uniform nor rectilinear; it varies incessantly, as well as the angular velocity of rotation. As the evaporation takes place principally at the surface of the water, the rotatory motion establishes itself round the axis which is perpendicular to this surface, and which passes through the centre of gravity of the body.

As, *ceteris paribus*, the emanation of the particles of the camphor is proportionate to the extent of its surface, and as the surfaces increase only as the squares, but the masses as the cubes of corresponding dimensions, the rapidity of the motion of the camphor must be greater in proportion as its volume is smaller, and consequently its motion must be accelerated in proportion as it evaporates; which coincides with experience.

Having established these propositions, which in my opinion comprehend the true theory of the motions of camphor upon water, let us return to the second part of the work of Citizen Benedict Prevost.

It includes a great number of experiments, in which we see inodorous substances produce, upon wet glass, the same phenomena as odorous, oily, or volatile substances. *Other experiments with bodies not odorous.*

If we spread a small piece of fine wet linen, of any figure, upon a china plate moistened with a thin layer of water, the water appears to recede all round it, forming a multitude of jets, and exhibiting the prismatic colours*.

* These colours probably result from the decomposition of the light by the small layer of water which surrounds the piece of linen, and which becomes still thinner by the retreat of the water.

If

If we pour upon the piece of linen, after having spread it upon the plate, some drops of water coloured with logwood, this water runs off in coloured streaks.

The same effects take place when we use a piece of fine white paper, not sized.

We obtain them equally with all the animal and vegetable substances, with the saline liquids and solutions; whether we place them, under the same circumstances, in contact with each other, or with water.

These phenomena take place not only upon a wet china-plate, but they are also observed upon surfaces of pottery, and many other substances.

From these facts, and several others analogous to them, Citizen Prevost draws the following inferences:

General results.

1. That all liquids possess the property of mutually repelling each other.

2. That all dry organised substances, which preserve any remains of organisation, exhale, whilst they imbibe water, an elastic fluid, which carries with it a part of this water, and repels that which surrounds it upon a piece of wet glass.

The first inference is contradictory to the general law of the mutual attraction of the molecules of matter.

As to the hypothesis of Citizen Prevost, concerning the formation of an elastic fluid, we shall remark, that, before he attributes phenomena to new causes, he ought to endeavour to account for them by those which are already known; to distinguish the effects produced by odorous substances from those presented by inodorous ones; and perhaps to establish in a more certain manner this repulsion of liquids by paper and linen; for this repulsion may very possibly be nothing more than an appearance produced by the flowing down of the water upon the inclined surface which these substances raise around themselves as they imbibe this liquid.

XIII.

*Case of a young Gentleman, who recovered his Sight when seven Years of Age, after having been deprived of it by Cataracts, before he was a Year old; with Remarks. By Mr. JAMES WARE, Surgeon.**

MASTER W. the son of a respectable clergyman, at Castlecary, in Somersetshire, was born in the year 1793; and, for many months, appeared to be a healthy perfect child: his eyes, in particular, were large and rather prominent. When about six months old, he began to cut his teeth; which was attended with great pain, and frequently with violent convulsive fits. About the end of his first year, a number of persons passing in procession near his father's house, accompanied with music and flags, the child was taken to see them; but, instead of looking at the procession, it was observed that, though he was evidently much pleased with the music, his eyes were never directed to the place from whence the sound came. His mother, alarmed by this discovery, was naturally led to try whether he could see silver spoons, and other glaring objects, which she held before him at different distances; and she was soon convinced, that he was unable to perceive any of them. A surgeon in the country was consulted, who, on examining the child's eyes, discovered an opacity in the pupils, which was so considerable, that he did not hesitate to pronounce there was a complete cataract in each. A description of the child's situation was then sent to me, with a request that I would point out those steps which its parents should pursue. The case was so evident, that I could not hesitate in saying, that the removal of the opaque crystalline humour, from the place it occupied behind the pupil, was the only method by which the child could obtain his sight; and, attached as I was, at that time, in all cases, to the operation of extracting the cataract, in preference to that of depressing it, I added, that I did not think he would be fit for the operation, until he was at least thirteen or fourteen years old. This advice being approved, all thoughts of assisting his sight were, for the present, relinquished. He

*History of the
infancy of a
child blind with
cataracts.*

* Philof. Transf. 1801, p. 382.

soon

History of the
infancy of a
child blind with
cataracts.

soon discovered a great fondness for music; his memory was very retentive of the little stories that were read or recited to him; and, in every way, it became evident that he had a mind capable of receiving information. As soon as he could speak, it was also observed, that when an object was held close to his eyes, he was able to distinguish its colour, if strongly marked; but, on no occasion, did he ever notice its outline or figure. In November, 1800, his parents took him to Bristol; whither they went for the purpose of seeing the works carried on in the school for the indigent blind in that city, and in order that they might ascertain whether their son, who was then arrived to his seventh year, could be taught any thing that would be useful or amusing. Here he very quickly learnt the art of making laces. But his parents, having brought him so far from home, thought it advisable to extend their plan, and make a visit to the metropolis, for the sake of giving me an opportunity of inspecting his eyes, and of hearing whether my opinion continued the same as that which I had written to them six years before. About a

Case of a boy
who recovered
his sight by
puncture of the
capsule of the
crystalline.

month previous to the time of their arrival, a Portuguese boy, fourteen years old, had been put under my care, who was in a similar situation; and, in this case, notwithstanding all the efforts I could use, I found it impossible to fix the eye, in order to extract the cataract, without employing a degree of force which might have been highly injurious. I therefore relinquished my intention of performing the operation in that way, and determined to make use of the couching needle; being prepared, either to depress the cataract with this instrument, if it was sufficiently solid for the purpose, or, if it was soft or fluid, (which I rather expected,) to puncture its capsule largely, so as to bring the opaque crystalline into free contact with the aqueous and vitreous humours. In order to fix the eye for this operation, I was not afraid to make use of a speculum oculi; since a pressure, which would have been highly dangerous in extracting the cataract, might be applied on the present occasion with perfect safety. Conformably to my expectation, the cataract was of a soft consistence; in consequence of which, I was not able to depress it, and contented myself with making a large aperture through the capsule, by means of which the crystalline was brought into
contact

contact with the other humours, a considerable part of it coming forwards, and shewing itself directly under the cornea.

This being the immediate result of the operation, it could not be expected that any improvement should be made in the sight of the patient at that time. In a few days, however, the opaque matter was wholly absorbed; the pupils became clear; and the lad recovered the sight of both his eyes.* Encouraged by the success which followed this operation, I was induced to retract the opinion which I had formerly sent to Master W.'s father, (which opinion I had given under the impression that the cataract should be extracted,) and I now proposed, that an attempt should be made to afford relief to one eye, at least, without further loss of time; this attempt, in the way above mentioned, being practicable with as much safety at his present age as at any future period; and, if it proved successful, it would give the young gentleman the benefit of vision five or six years sooner than his friends had been encouraged to expect, by my former letter on this subject. They were naturally much pleased with this alteration in my advice; and the child himself appearing to possess a great degree of fortitude, I performed the operation on the left eye, on the 29th of December last, in the presence of Mr. Chamberlayne, F. A. S. Doctor Bradley, of Baliol College, Oxford, and Mr. Platt, surgeon, in London. It is not necessary, in this place, to enter into a description of the operation. It will be sufficient to say, that the child, during its performance, neither uttered an exclamation, nor made the smallest motion, either with his head or hands. The eye was immediately bound up, and no inquiries made on that day with regard to his sight. On the 30th, I found that he had experienced a slight sickness on the preceding evening,

The same operation performed on the subject of this paper, at about eight years old.

* It should be remarked, that the sight obtained by children who are born with cataracts, is seldom so perfect as that which those recover, after the operation, who are afflicted with the disorder later in life. In consequence either of some remaining opacity in the crystalline capsule, which hinders the free admission of the rays of light, or of a greater tenuity in the remaining humours of the eye, children require, in general, a much deeper convex glass to enable them to see minute objects; and, at the same time, they are obliged to hold them much nearer their eyes than older persons.

but

He gained his
fight,

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and a white mug.

Other experi-
ments, by which
it appeared that
he knew dis-
tances and fi-
gures with much
precision ;

but had made no complaint of pain, either in his head or eye, On the 31st, as soon as I entered his chamber, the mother, with much joy, informed me that her child could see. About an hour before my visit, he was standing near the fire, with a handkerchief tied loosely over his eyes, when he told her that under the handkerchief, which had slipped upward, he could distinguish the table by the side of which she was sitting : it was about a yard and a half from him ; and he observed that it was covered with a green cloth, (which was really the case,) and that it was a little farther off than he was able to reach. No further questions were asked him at that time ; as his mother was much alarmed, lest the use thus made of his eye might have been premature and injurious. Upon examination, I found that it was not more inflamed than the other eye ; and the opacity in the pupil did not appear to be much diminished. Desirous, however, to ascertain whether he was able to distinguish objects, I held a letter before him, at the distance of about twelve inches, when he told me, after a short hesitation, that it was a piece of paper ; that it was square, which he knew by its corners ; and that it was longer in one direction than it was in the other. On being desired to point to the corners, he did it with great precision, and readily carried his finger in the line of its longest diameter. I then shewed him a small oblong band-box covered with red leather, which he said was red and square, and pointed at once to its four corners. After this, I placed before him an oval silver box, which he said had a shining appearance ; and, presently afterwards, that it was round, because it had not corners. The observation, however, which appeared to me most remarkable, was that which related to a white stone mug ; which he first called a white basin, but, soon after, recollecting himself, said it was a mug, because it had a handle. These experiments did not give him any pain ; and they were made in the presence of his mother, and of Mr. Woodford, a clerk in his Majesty's Treasury. I held the objects at different distances from his eye, and inquired very particularly if he was sensible of any difference in their situation ; which he always said he was, informing me, on every change, whether they were brought nearer to, or carried further from him. I again inquired, both of his mother and himself, whether he had ever, before

this

this time, distinguished by sight any sort of object; and I was assured by both that he never had, on any occasion; and that, when he wished to discover colours, which he could only do when they were very strong, he had always been obliged to hold the coloured object close to his eye, and a little on one side, to avoid the projection of the nose. No further experiments were made on that day. On the 1st of January, I found that his eye continued quite free both from pain and inflammation, and that he felt no uneasiness on the approach of light. I shewed him a table knife; which at first he called a spoon, but soon rectified the mistake, giving it the right name, and distinguishing the blade from the handle, by pointing to each as he was desired. He afterwards called a yellow pocket-book by its name, taking notice of the silver lock in the cover. I held my hand before him; which he knew, but could not at first tell the number of my fingers, nor distinguish one of them from another. I then held up his own hand, and desired him to remark the difference between his thumb and fingers; after which, he readily pointed out the distinctions in mine also. Dark-coloured and smooth objects, were more agreeable to him than those which were bright and rough. On the 3d of January, he saw, from the drawing-room window, a dancing bear in the street; and distinguished a number of boys that were standing round him, noticing particularly a bundle of clothes which one of them had on his head. On the same evening, I placed him before a looking glass, and held up his hand: after a little time he smiled, and said he saw the shadow of his hand, as well as that of his head. He could not then distinguish his features; but, on the following day, his mother having again placed him before the glass, he pointed to his eyes, nose, and mouth, and seemed much gratified with the sight.

Having thus stated the principal observations that were made by Master W. I shall now make a brief comparison between this statement, and that which is given in the XXXVth volume of the Philosophical Transactions, of Mr. Chefelden's patient, who was supposed to be born blind, and obtained his sight when he was between thirteen and fourteen years old.

Comparison of
this statement
with the famous
instance of Che-
felden.

It

It should be observed, that though Master W. was six years younger than Mr. Cheselden's patient, he was remarkably intelligent, and gave the most direct and satisfactory answers to every question that was put to him. Both of them, also, if not born blind, lost their sight so very early, that, as Mr. Cheselden expresses it, "they had not any recollection of having ever seen."

My first remark is, that, contrary to the experience of Mr. Cheselden's patient, who is stated "to have been so far from making any judgment of distance, that he thought all objects touched his eyes, as what he felt did his skin." Master W. distinguished, as soon as he was able to see, a table, a yard and a half from him; and proved that he had some accuracy in his idea of distance, by saying, that it was a little further off than his hand could reach. This observation, so contrary to the account we have received of Mr. Cheselden's patient, would have surprised me much more than it did, if I had not previously, in some similar instances, had reason to suspect that children, from whom cataracts had been extracted, had a notion of distance the first moment they were enabled to see.

Other instances
differing from
Cheselden.

In the instance particularly of a young gentleman from Ireland, fourteen years old, from each of whose eyes I extracted a cataract, in the year 1794, in the presence of Dr. Hamilton, physician to the London Hospital, and who, before the operation, assured me, as did his friends, that he never had seen the figure of any object, Dr. Hamilton and myself were much astonished by the facility with which, on the first experiment, he took hold of my hand at different distances, mentioning whether it was brought nearer to, or carried further from him, and conveying his hand to mine in a circular direction, that we might be the better satisfied of the accuracy with which he did it. In this case, however, and in others of a like nature, although the patients had been certainly been blind from early infancy, I could not satisfy myself that they had not, before this period, enjoyed a sufficient degree of sight to impress the image of visible objects on their minds, and to give them ideas which could not afterwards be entirely obliterated. In the instance of Master W. however, no suspicion of this kind could occur; since, in addition to the declaration of himself and his mother, it was proved by the testimony of the surgeon

who examined his eyes in the country, that the cataracts were fully formed before he was a year old. And I beg leave to add further, that on making inquiries of two children, between seven and eight years of age, now under my care, both of whom have been blind from birth, and on whom no operation has yet been performed, I find that the knowledge they have of colours, limited as it is, is sufficient to enable them to tell whether coloured objects be brought nearer to, or carried further from them, for instance, whether they are at the distance of two inches or four inches from their eyes; nor have either of them the slightest suspicion, as is related of Mr. Cheselden's patient, that coloured objects, when held before them, touch their eyes.

But the judgment which Master W. formed of the different distances of objects, was not the only instance in which he differed from Mr. Cheselden's patient; who, we are informed, "did not know the figure of any thing, nor any one thing from another, however different in shape and magnitude;" for Master W. knew and described a letter, not only as white, but also as square, because it had corners; and an oval silver box, not only as shining, but also as round, because it had not corners: he likewise knew, and called by its name, a white stone mug, on the first day he obtained his sight, distinguishing it from a basin, because it had a handle. These experiments were made in the presence of two respectable persons, as well as myself; and they were several times repeated, to convince us that we could not be mistaken in them. I mention the circumstance, however, with much diffidence, being aware that the observations not only differ from those that are related of Mr. Cheselden's patient, but appear, on the first statement, to oppose a principle in optics, which I believe is commonly and justly admitted, that the senses of sight and feeling have no other connection than that which is formed by experience; and, therefore, that the ideas derived from feeling can have no power to direct the judgment, with respect either to the distance or form of visible objects. It should be recollected, however, that persons who have cataracts in their eyes, are not, in strictness of speech, blind, though they are deprived of all useful sight. The instances I have adduced prove, that the knowledge they have of colours is sufficient to give them some idea of distance, even in their darkest

darkest state. When, therefore, their sight is cleared by the removal of the opaque crystalline, which intercepted the light, and the colour of objects is thereby made to appear stronger, will it be difficult or unphilosophical, to conceive that their ideas of distance will be strengthened, and so far extended as to give them a knowledge, even of the outline and figure of those objects with the colour of which they were previously acquainted?

Chirurgical observations and remarks.

The case which I have here related appears to deserve notice, not only on account of the observations that were made by the patient on recovering his sight, but also on account of the hint which it affords to surgeons, relative both to the mode in which the cataract may best be removed, when children are born with this disorder, and the time when it is most proper to perform the operation.

In what cases depression of the cataract may be preferable to extraction.

The Baron de Wenzel, in his ingenious Treatise on the Cataract, with great force of reasoning, deduced from the long and successful experience of his father and himself, recommends, in all cases of this disorder, without making any exceptions, the operation of extraction, in preference to that of depression; and I believe it is now generally acknowledged by medical men, that in the more common cases, his decision as to the mode of operating is perfectly well founded. The Baron admits that the operation is not so certain a cure in children as it is in persons of a more advanced age; both on account of their untractableness, and because, in them, the opacity of the crystalline is not unfrequently accompanied with an opacity in the capsule that contains it. On these accounts, when children are born with this disorder, he advises to postpone the operation, until they are old enough to be made sensible of the loss they sustain by the want of sight, and have firmness of mind to submit patiently to the means that are requisite in order to obtain it. Influenced by this opinion of the Baron, and believing the operation of extraction to be so much superior to that of depression, that the latter ought not, on any occasion, to have the preference, I have given advice, in the cases of a considerable number of children who were born with this disorder, to postpone every attempt to relieve them, until they were thirteen or fourteen years old. Prior to this time, it did not appear to me that children could be depended upon to submit, with due steadiness, to the repeated introduction of instruments,

instruments, which is sometimes necessary in extracting the cataract; and, even at this age, the eyes of some are so small, and in such a constant rolling motion, that it is almost impossible properly to accomplish the operation. The Portuguese lad, whose case has been related, afforded an instance of this kind; and I consider it as a fortunate circumstance that it came under my notice, since, in some degree, it may be said to have obliged me to examine, more attentively than I had before done, the advantages and disadvantages of the operation of depression; which operation, being more easy to perform than that of extraction, has certainly this advantage in the cases of children, (to which alone I here advert,) that it may be performed with equal safety when they are only seven years of age, as it may at any subsequent period of their lives.

It is well known that the late Mr. Pott, who published his remarks on the cataract in the year 1775, was a strenuous advocate for this operation; and, though he appears to me to have much under-rated the advantages of extraction, it must be allowed that he makes many just and highly pertinent observations on the use of the couching needle, in those cases where the cataract is soft, or fluid. Mr. Pott considered this as a very common state of the disorder; and does not make any distinction between the cataract when it attacks grown persons, and when children are born with it. In the former case, experience inclines me to believe, that the cataract is very rarely fluid, or even soft; whereas, in the latter, I have always found it, agreeable to the observation of the Baron de Wenzel, in one or other of these states. Although, therefore, in the case of grown persons, the operation of extraction appears to me to have very great advantages over that of depression, yet, in the case of children, I can readily accede to almost the whole that Mr. Pott advances in favour of depression. If the couching needle be passed in the way in which it is usually introduced to depress the cataract, and thereby a large aperture be made in the capsule of the crystalline, (which operation may be performed with perfect safety, and with very little pain to the patient, whilst the eye is fixed with a speculum oculi,) the opaque crystalline, being thus brought into contact with the aqueous and vitreous humours, will, in a shorter or longer space of time, according to its degree of soft-

Pott favoured the practice of depression.

Reasons in support of Pott.

preceding pages, in the presence of Mr. Scott, surgeon, in St. Alban's-street. The operation gave her no pain; and, in the course of a few days, the opacity was evidently diminished, particularly in the right eye, the patient discovering the colour of objects more plainly than before, but being still unable to distinguish their figure. At the end of a month, finding no further improvement in her vision, it appeared to me most probable that the remaining opacity was situated in the capsules. I therefore determined to extract either a part or the whole of each of them. The incisions of the cornea were made in the usual manner; after which, I punctured the anterior parts of both the capsules with the sharp end of a gold curette. The punctures became immediately transparent, without affording an issue to the liquor Meibomii, or any other humour. From hence it seems evident, that nothing was contained within the capsules, or, in other words, that the crystalline humours were absorbed; and it appears to me highly probable, that their absorption had been occasioned by the previous operation of puncturing their capsules with the couching needle. I dilated the new punctures with the end of the curette; and afterwards, being still afraid that the apertures in the capsules might not be large enough to admit a sufficient number of the rays of light, I removed a portion of each of them with a small forceps. This was accomplished in the left eye, without occasioning the discharge of any part of the vitreous humour; and, in the right, the quantity of this humour that came away was very small. In the course of a week, the inflammation that followed the operation was nearly removed; a large portion of both pupils was quite clear; and the young lady distinguished objects with quickness and precision.

SCIENTIFIC

SCIENTIFIC NEWS.

NATIONAL INSTITUTE OF FRANCE.

Extract of the Report of the Transactions of the Class of Mathematical and Physical Sciences, during the last Trimestre of the Year 9. French National Institute.

Mathematical Part. By CITIZEN DELAMBRE.

Memoir on the Equilibrium of Arches. By CIT. BOSSUT. Bossut on arches.

AS the key stones of which an arch is composed, support themselves by mutual equilibration, and remain suspended without the aid of any support beneath, their whole force directing itself against the buttresses; in order to ensure the duration of an arch, it is not sufficient that all its key-stones be in equilibrio; but it is necessary that its supports at the ends should oppose a resistance adequate to the force which it exerts in order to overthrow or break them in pieces.

The enquiry respecting the means proper for preventing their being overthrown, constitutes what is termed the problem of the lateral pressure of arches. Several geometricians of the last century had already occupied themselves with this subject, but they had intirely neglected to inquire into the means of preventing these supports from being crushed.

C. Bossut undertook, in the year 1770, to treat the question in the most general manner; as well for simple arches as domes. He has investigated whatever relates to the figure and the lateral pressure of vaults. His memoirs have been printed in the volumes of the Academy of Sciences for the years 1774 and 1776.

After many new observations and various experiments, which may be of the greatest practical utility, C. Bossut has resumed the subject of his two memoirs. He has now modelled them by simplifying his calculations in several places, and has made a number of additions relative both to theory and practice; so that the whole, in its present state, constitutes a work that may be considered as original.

Comets.—C. Messier has read an account of the comet Comet of July 12, 1801, which he discovered on the 23d of Messidor last, about half an hour

hour after eleven in the evening: its light was very feeble; in 41 minutes of time it moved 24 min. 40 sec. of right ascension direct, and 6 min. 38 sec. north declination decreasing. The same comet was seen on the same day, and almost at the same instant by Citizens Mechain and Bouvard; the latter of whom had even observed it at the meridian at $11^h 57^m 49^s$ true time. The right ascension was 111 deg. 15 min. and the north declination 69 deg. 30 min.

C. Pons observed it on the same day at Marseilles, and he had even perceived it on the preceding day; but the clouds did not at that time permit him to ascertain, by regular observations, whether it was a comet, or merely a nubecula.

Summer solstice,
1801.

Observation of the summer solstice of the year 9.—Citizen Duc-la-Chapelle, associate, has communicated to us the result of the observations, made by him at Montauban, in order to determine the solstitial height of the sun, and the obliquity of the ecliptic.

From an average of nine days observations, he finds 23 deg. 28 min. 9 sec. for the apparent obliquity, taking 15 min. 48 deg. for the semi-diameter of the sun, and 44 deg. 0 min. 52 sec. for the latitude of his observatory.

Lalande on the
fixed stars.

Remarks concerning the 50,000 stars, of which observations have been published by Citizen Jerom Lalande.—C. Lalande, in the preface to his *Histoire Céleste*, had noticed the existence of many blank spaces in the heavens, many changeable stars, and many red stars. He now resumes these subjects in a more circumstantial manner, in a memoir accompanied with tables.

By blank spaces, (*d'espèces vuides*) Citizen Lalande understands in this memoir, those spaces in which no stars of the 9th degree of magnitude are to be perceived. These are the smallest that can easily be distinguished with an achromatic telescope of 67 millimeters aperture, in which light is admitted to illuminate the wires.

It is not to be doubted that by excluding all light from surrounding objects, and employing the most powerful telescopes, the blanks properly so called would be found to be considerably diminished; perhaps even there is not a single spot in the firmament, towards which we could direct a telescope, without perceiving a great number of stars, though below the ninth degree of magnitude, and consequently too small to be of any use in astronomy. C. Lalande gives the catalogue of

all these blank spaces; that is to say, his table comprehends the right ascension and declination of the middle of each of these spaces.

The changeable stars are comprehended in a second table; they are thirty one in number. There are not more than twelve, the periods of which are known; but there are several others which are diminished so as to disappear. By following them attentively, we may determine the time which elapses between two successive disappearances; and this is a species of observation which Citizen Lalande proposes to the curiosity of those who, having only indifferent instruments, wish nevertheless to be useful to astronomical science.

A third table presents thirty-three stars of a red colour. Since the year 1756, Mayer had remarked this colour in the 19th of Pisces, which he designates in his registers of observations by the epithet of *Rubicunda*, as he finds by the copy, which Prof. Lichtenberg has sent of all the observations made by Mayer, on the day when he observed Herschel's planet. Mitchell and Baillie suppose, that the colours of the stars may depend either on the different intensity of their fire, or the degree of their inflammation, and that the red colour may indicate a fire which is in a diminishing state. According to this hypothesis, it would be a matter of importance to examine the changes of colour which take place in the stars. Be that as it may, these variations, if they exist, are certainly extremely slow; for the different shades of colour which are remarked at the present day in Antares, Arcturus, Aldebaran, Sirius, and the Lyre, existed in the age of Ptolemy.

Extract of a memoir on the degree of magnetism which blades of steel of different thickness acquire, and on some results relative to the needles of the compass, by CITIZEN COULOMB.—Almost all the magnetic phenomena may be reduced to calculation, if we suppose two magnetic fluids to exist in the steel, in each of which the molecules repel each other in the inverse ratio of the square of the distances, and attract the particles of the other fluid in the same ratio. This law has been proved by C. Coulomb in the Memoires of the Academy of Sciences for 1786, according to experiments which appear to be decisive.

When the steel is in its natural state, and has not been touched with the load stone, the two fluids are neutralized by each other; that is to say, they keep themselves in equilibrio, and

and exert no action, since one of the fluids attracts a magnetic particle with the same force as that with which the other repels it.

When the fluids are separated, they tend to unite and neutralize each other; and they would actually unite, were there not a coercive force that opposed this union. This force may be either the adhesion of the molecules of the fluid to the steel, or the friction which they undergo in passing from one point to another.

The author has proved, *Memoires de l'Academie des Sciences pour 1787*, page 491, that whether we suppose the two fluids to be separated and carried each to one extremity of the blade, or that they are only displaced in each particle of the steel, the result of the calculation will be the same. He has proved at the same time, that this latter supposition is the only one which can be made to agree with the facts.

In the present memoir the author endeavours to determine the magnetic state of several blades united successively one above the other; or, which amounts to the same thing, he endeavours to determine the powers of different blades relative to their thickness.

New Planet.

New planet between Mars and Jupiter.

Mr. Piazzi, of the University of Palermo, discovered on the 1st of January, 1801, a star which appears to be a new planet. By observations repeated for several succeeding days he concluded, that its orbit is not likely to be parabolic, but agrees best with the hypothesis of a circle, the radius of which appears to be 2,6362 of the earth's mean distance, and consequently its position will be between Mars and Jupiter. Its bulk appears to be about $1\frac{1}{4}$ that of the earth. He has assigned to it the name of *Ceres Ferdinandia*, being the names of the ancient divinity of Sicily, and of its present sovereign the founder of the Observatory at Palermo. An account has been lately presented to the Royal Society. At present I give the following particulars from a paper in German.

OBSERVATIONS.

OBSERVATIONS.

1801	Med. Time.	Geo. Long.	Geo. Lat.	Right As.	Decl.
June 20	13. 4	101. 45	3. 26 N	103. 6 40	26 22 10 N
July 17	1. 43	113. 3	4. 6	115. 38 50	25 32 35
Aug. 12	10. 54	124. 21	4. 51	127. 56 5	23 51 10
Sept. 7	16. 19	135. 23	5. 41	139. 44 30	21 38 25
12	22. 0	137. 40	5. 52	142. 2 20	21 8 10
18	3. 0	139. 50	6. 3	144. 17 0	20 37 0
23	8. 0	141. 58	6. 15	146. 29 20	20 6 25
28	13. 0	144. 5	6. 27	148. 40 0	19 35 0
Oct. 3	17. 41	146. 9	6. 40	150. 47 0	19 4 40
8	22. 0	148. 12	6. 53	152. 50 50	18 33 40
14	3. 0	150. 12	7. 8	154. 55 30	18 4 40
19	7. 0	152. 11	7. 22	156. 56 20	17 34 20
24	11. 0	154. 8	7. 37	158. 55 0	17 5 10
29	14. 45	156. 3	7. 53	160. 51 30	16 37 0
Nov. 3	18. 0	157. 56	8. 9	162. 45 40	16 9 0
8	22. 0	159. 48	8. 26	164. 38 50	15 41 50

Place of Ascending Node	S. 20. 58. 30
Inclination of the Orbit	10. 47. 0
Place of the Aphelion	2. 8. 59. 37
Time of passage through the Aphelion Jan. 1.	
1801	1,3328
Eccentricity	0,0364
Log. of the half great Axis	0,4106986
Time of the Siderial period	4,13 years.

ACCOUNT OF BOOKS OF SCIENCE.

Philosophical Transactions of the Royal Society of London, for the Year 1801. Part the Second. Quarto, 214 Pages, with Eighteen Plates. London, sold by Elmly.

THIS Part contains:—1. A Historical and Anatomical Description of a doubtful Amphibious Animal of Germany, called by Laurenti, *Proteus Anguinus*. By Charles Schreibers, M. D. of Vienna.—2. Observations tending to investigate the Nature of the Sun, in order to find the Causes or Symptoms of

Royal Society
of London.

Royal Society
of London.

of its variable Emission of Light and Heat; with Remarks on the Use that may possibly be drawn from Solar Observations. By William Herschel, LL. D. F. R. S.—3. Observations on the Structure and Mode of Growth of the Grinding Teeth of the Wild Boar, and Animal Incognitum. By Everard Home, Esq. F. R. S.—4. Account of some Experiments on the Ascent of the Sap in Trees. By Thomas Andrew Knight, Esq.—5. Additional Observations tending to investigate the Symptoms of the variable Emission of the Light and Heat of the Sun; with Trials to set aside Darkening Glasses by transmitting the Solar Rays through Liquids; and a few Remarks to remove Objections that might be made against some of the Arguments contained in the former Paper. By William Herschel, LL. D. F. R. S.—6. On an improved Reflecting Circle. By Joseph de Mendoza Rios, Esq. F. R. S.—7. Observations and Experiments upon Dr. James's Powder; with a Method of preparing, in the Humid Way, a similar Substance. By Richard Chenevix, Esq. F. R. S. M. R. I. A.—8. Case of a young Gentleman, who recovered his Sight when seven Years of Age, after having been deprived of it by Cataracts, before he was a Year old; with Remarks, By Mr. James Ware, Surgeon.—9. An Account of some Galvanic Combinations, formed by the Arrangement of single Metallic Plates and Fluids, analogous to the new Galvanic Apparatus of Mr. Volta. By Mr. Humphry Davy, Lecturer on Chemistry in the Royal Institution.—10. A Continuation of the Experiments and Observations on the Light which is spontaneously emitted from various Bodies; with some Experiments and Observations on Solar Light, when imbibed by Canton's Phosphorus. By Nathaniel Hulme, M. D. F. R. S. & A. S.—11. Experiments on the Chemical Production and Agency of Electricity. By William Hyde Wollaston, M. D. F. R. S.—12. Farther Observations on the Effects which take place from the Destruction of the Membrana Tympani of the Ear; with an Account of an Operation for the Removal of a particular Species of Deafness. By Mr. Afley Cooper.

A Treatise

A Treatise on Astronomy, in which the Elements of the Science are deduced in a natural Order, from the Appearances of the Heavens, to an Observer on the Earth; demonstrated on Mathematical Principles, and explained by an Application to the various Phenomena. By Olinthus Gregory, Teacher of Mathematics, Cambridge. 8vo. 522 Pages, with Nine Plates. London, sold by Kearsley.

Elements of Chemistry. By J. Murray, Lecturer on Chemistry, Materia Medica, and Pharmacy. 8vo. 2 vols. 692 pages. Edinburgh printed. Sold by Longman and Rees, London.

Observations on the Winds and Monsoons, illustrated with a Chart, and accompanied with Notes, Geographical and Meteorological. By James Capper, formerly Colonel, and Comptroller-General of the Army and Fortification Accounts on the Coast of Coromandel. Quarto. 234 Pages. Debrett.

Accounts of the three last mentioned Works will appear in our next.

Über den Werth und Nutzen der Runkelrüben.—On the Value and Use of the White Beet-Root. By J. G. W. 1801. 29 Pages. Glogau.

This pamphlet contains minute instructions for preparing sugar, syrup, coffee, brandy, rum, and arrack, from the white beet-root (*beta cicla*). In order to obtain sugar from the beet-root, the author directs the roots to be cut into slices, to express the juice, to boil it down to a strong consistence, and to suffer it to crystallize. The residue, from which the juice has been expressed, may be used for obtaining brandy, but it is better to boil the beet-root to reduce it to a pulp, suffer it to ferment, and then submit it to distillation. In order to deprive the spirit of the peculiar flavour of the root, it must be rectified over charcoal powder. Eighty pounds of beet-root yielded eight quarts of brandy. To obtain rum, the roots are likewise boiled, the juice expressed, and mixed with charcoal powder, evaporated to one-third, then suffered to ferment, and afterwards distilled with the charcoal powder. In order to obtain arrack, the juice is to be evaporated to one-half, then suffered to ferment, and distilled.

Recherches

Des-Mortiers on *Recherches sur la Décoloration spontanée du Bleu de Prusse, &c.*—
Prussian Blue.

Inquiries into the spontaneous Loss of Colour in Prussian Blue, and the spontaneous Return of its Colour; read at a private meeting of the Free Society of Sciences, Literature, and Arts, at Paris, by U. R. T. le Bowyer-des-Mortiers, Member of the Society. Paris. 8vo. 32 p. 1801.

A person had made for sale a pretty considerable quantity of blue paint, consisting of a mixture of white lead, Prussian blue, and nut oil. To prevent its drying he covered it with water some inches deep, and set it by till wanted. After a certain time, a person coming to buy some of the paint, he was much surprised to find it all white, except at the surface, where the colour was well preserved. He was preparing to add fresh Prussian blue to the paint, when, on grinding it in the open air, without any addition, he saw the colour return of itself, and on continuing this operation, it became as deep a blue as at first. He then covered it with oil, supposing it would keep better than under water; but he was disappointed, for the colour disappeared a second time throughout the whole mass.

The Society of Physic and the Arts at Nantes, to whom some of this paint was carried in its white state, spread part of it on writing paper, part on wood, and part on the wall of a window. After a longer or shorter time, the colour of the paint re-appeared with all its lustre, but that on the paper was most slowly restored.

What is the cause of this phenomena? Is it the oil, which, by undergoing a change, destroys the colour of the Prussian blue? Is it the air altogether, or one of its constituent principles, or any other substance mixed or dissolved in it, that restores the colour? These are the questions which the author endeavours to answer, in publishing the different interesting experiments he has made on the subject, from which he deduces the following conclusions.

1. The loss of colour in the paint is not owing to the decomposition of the oil, but to a change of surfaces, occasioned by the subsiding of the mass, and by the extinction of the luminous globules in the minute laminae and in the pores of the colouring substance. 2. Neither the air altogether, nor one of its constituent principles, nor any thing it contains, is necessary

cessary to restore the colour; for it revives *in vacuo* as well as in the open air. 3. Caloric, without the contact of light, is detrimental to the return of the colour, instead of contributing to it, and even destroys it. 4. Finally, the intestine motion of its parts, however exerted, is sufficient to re-produce the colour more or less speedily, according as there are more or less light and motion.

Essai sur le Calorique.—An *Essay on Caloric; or, Inquiries into the Physical and Chemical Causes of the Phenomena exhibited by Bodies subjected to the Action of the Ignecous Fluid, with new Applications of them respecting the Theory of Respiration, Animal Heat, the Origin of Volcanic Fires, &c.; to which are added, An Essay on the Anomalies of Chemical Affinities; Experiments and Observations on Bell-metal; and a Description of the famous Alum Mine of Souvignaco, in Istria, and of the Processes employed for extracting and purifying the native Alum:* by Joseph-Mary Socquet, M. D. formerly Physician to the Army. Paris, 8vo. 473 p. Price 5fr. 1801.

Natural philosophers are yet far from being agreed on the nature of caloric, and the phenomena ascribed to it. In this volume Dr. S. has published eight essays on the subject.

The 1st treats of caloric considered in its chemical and physical relations to other bodies, whence may be deduced the principal phenomena they exhibit when subjected to the action of this fluid; as their capacity for caloric, their dilatation, fusion, gasification, tendency to equilibrium of temperature, &c.

In the 2d the author turns his attention to the cause of the perpetual production of heat by the friction of bodies, and of the consequences which Count Rumford deduced from his own experiments on the subject.

In the 3d he treats of the conducting power of liquids for caloric.

The 4th contains new views on respiration, and the cause of the production of heat in warm-blooded animals.

The subject of the 5th is the nature of volcanic fires, the causes of which the author deems independent of any local combustion, produced by the decomposition of water or of any other oxide, or by submarine or subterranean currents of air.

The

Socquet on Ca-
storic.

The 6th contains seven cases of chemical affinity:—1. Pre-disposing affinities, as they are called: 2. The decomposition of sulphate of iron by caustic ammoniac: 3. The decomposition of muriate of soda by sulphate of magnesia, or of the zero of Reaumur: 4. The reciprocal decomposition of alkaline phosphates by carbon, and of alkaline carbonates by phosphorus: 5. The decomposition of water in the solution of iron by diluted sulphuric acid; and the decomposition of the acid, not of the water, in dissolving quicksilver by the same menstruum: 5. The precipitation of free sulphuric acid, by mixing the two sulphates of mercury and ammoniac: 7. Whence comes it, that those portions of solvents which are last fixed in their bases easily separate from them, while the first portions, though of the same nature, and fixed in the same substances, adhere to them much more forcibly?

The author's residence in Italy having given him an opportunity of travelling over almost all the provinces of the quondam state of Venice, and some other countries dependent on Austria, he visited a great number of productive and interesting manufactories. The extensive alum mine of Souvignaco, in Istria, of which there was no description extant, probably on account of its very remote and wild situation, and perhaps because it is of no ancient date, afforded him some curious particulars respecting the disposition and nature of the ore, the simple, ingenious, and partly new processes followed there, and the theory of the various phenomena they exhibit. These are the subjects of the 7th essay.

In the 8th Dr. S. gives a summary of his experiments on the extraction of pure copper from bell-metal, the results of which had already been published in several Italian journals; together with some other operations in the large way, which he had executed at Venice on different objects of art, particularly the separation of soda from common salt*. The corrections, or rather additions, proposed by the Chev. Napione to the processes already known for decomposing the different kinds of bronze, were too interesting, for our author to omit making them known at large; particularly as he had an opportunity of convincing himself of their utility, by repeating the experiments of that learned mineralogist, at Turin.

* See *Opuscoli scelti sulle Scienze e sulle Arti: da Carlo Amoreti: tom. xx.* Milan.

Atlas de la Partie Meridionale de l'Europe, &c.—*An Atlas of the Southern Part of Europe, constructed to the Meridian of Paris, and consisting of forty Sheets, which, being on the same Scale, may be pasted together, so as to form the most comprehensive Map that has ever yet been published of the different States it includes: by C. Chanlaire, Member of the Lyceum, and one of the Authors of the National Atlas of France.* Paris. 1801. *Chanlaire's Atlas of the South of Europe.*

This map is remarkable for the accuracy of its details, and may supply the place of a great many maps of single countries.

Mémoires d'Agriculture, &c.—*Memoirs of Agriculture, and Rural and Domestic Economy, published by the Agricultural Society of the department of the Seine, and printed by order of the Prefect of the department.* Vol. I. Paris. 8vo. 447 p. *Memoirs of the Agricultural Society of the department of the Seine.*
1 plate. Price 4fr.

An account of this work will appear in our next.

BOOKS OF SCIENCE,

Imported by Deboffe, of Gerrard-Street, Soho.

ANNUAIRE de la Republique Françoise, par le Bureau des Longitudes, pour l'An ix. Par. An. viii. in 18. br. 1s. 3d. *Books of science imported.*

Carnot de la Correlation des Figures de Geometrie. Par. 1801. 8vo. Fig. br. 7s.

Clairault Elém. d'Algebre. 6th Edit. 2 Vol. 8vo. Par. 1801. Fig. br. 14s.

Essais sur la Ligne Droite et les Courbes du Second Degré par François. 8vo. Par. 1801. 4s.

Gallitzin Recueil de Noms, &c. appropriés en Mineralogie suivi d'un Tableau Mythologique. Brunswick, 1801. 4to. br. 12s.

Haüy Traité de Mineralogie publié par le Conseil des Mines. Par. 1801. 4 Vol. avec Atlas. 2l. 2s.

Memoires

Books of science Memoires de l'Institut National, 3e livraison An. ix.
 Imposted. 3 Vol. 4to. Fig. cart. 9l. 3s.

Mémoires des Sociétés savantes de la République Française,
 par Prony, Parmentier, Duhamel, &c. 4to. pour l'Année.
 1l. 16s.

Monge, Traite Elémentaire de Statistique Par. An. ix. 8vo.
 Fig. br. 6s.

Olivarius le Nord, Littéraire, Physique, Politique et Morale
 Ouvrage periodique publié tous les 3 Mois, les 16 Premiers
 Cahiers. 8vo. br. 2l.

O'Reily Essai sur le Blanchiment. Par. 1801. 8vo.
 Fig. br. 7s. 6d.

Pallas Observations faites dans un Voyage enterpris dans
 les Gouvernemens Meridionaux de l'Empire de Russie, pen-
 dant les Années 1793 & 1794. Leipz. 1801. Tom. 2. 4to.
 Atlas. 4l. 14s. 6d.

Philosophie de Kaunt, ou Principes fondamentaux de la
 Philosophie transcendentalc, par Villiers. Metz. 801.
 8vo. br. 6s.

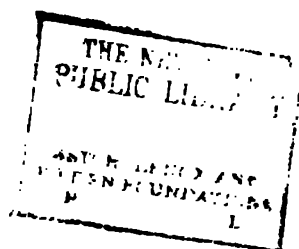
Plans & Dessins tirés de la Belle Architecture, ou Repre-
 sentations d'Edifices executés ou projetés, en 115 Planches
 par Stieglitz Londr. 1801. Fol. Cart. 9l. 9s.

Principes d'Economie Politique: Ouvrage couronné par
 l'Institut Nationale, par Canard. Par. 1801. 3s.

Traite Elémentaire de Physique par Libes, Professeur aux
 Ecoles Centrales de Paris. Par. 1801. 3 Vol. 8vo. Fig.
 br. 1l. 4s.

Voyage dans l'Inde et au Bengal, pendant les Années 1789
 et 1790, par Grandpre. Par. 1801. 2 Vol. 8vo. Fig.
 br. 14s.

Voyage en Syrie, No. 20. Proof Plates, 2l. 12s. 6d.—Not
 Proof, 1l. 15s.



Herschel's observations on the Sun.

Fig. 1.



Fig. 2.



Fig. 3.



Fig. 4.



Fig. 5.



Fig. 6.



Fig. 7.



Fig. 8.



Fig. 9.



Fig. 10.



Fig. 11.



Fig. 12.



Fig. 13.



Fig. 14.



Fig. 15.

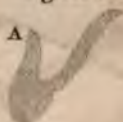


Fig. 16.



Fig. 18.

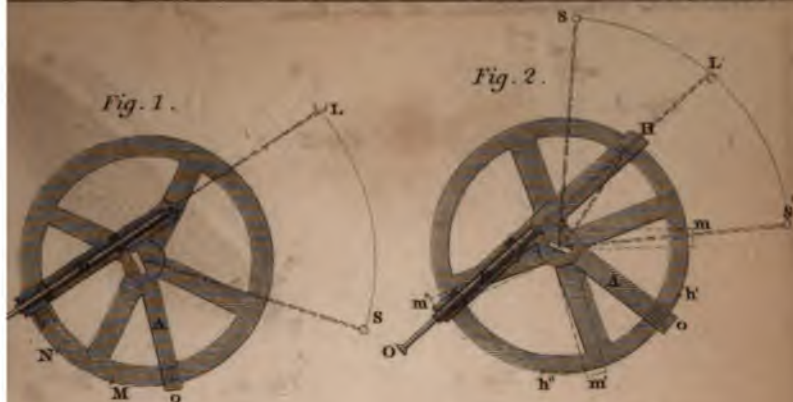


Fig. 17.



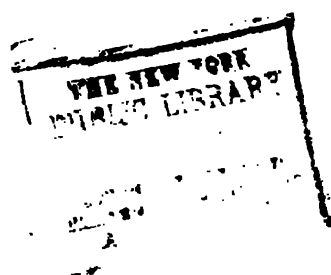
Fig. 19.





Mendoza's New Circular Instrument.

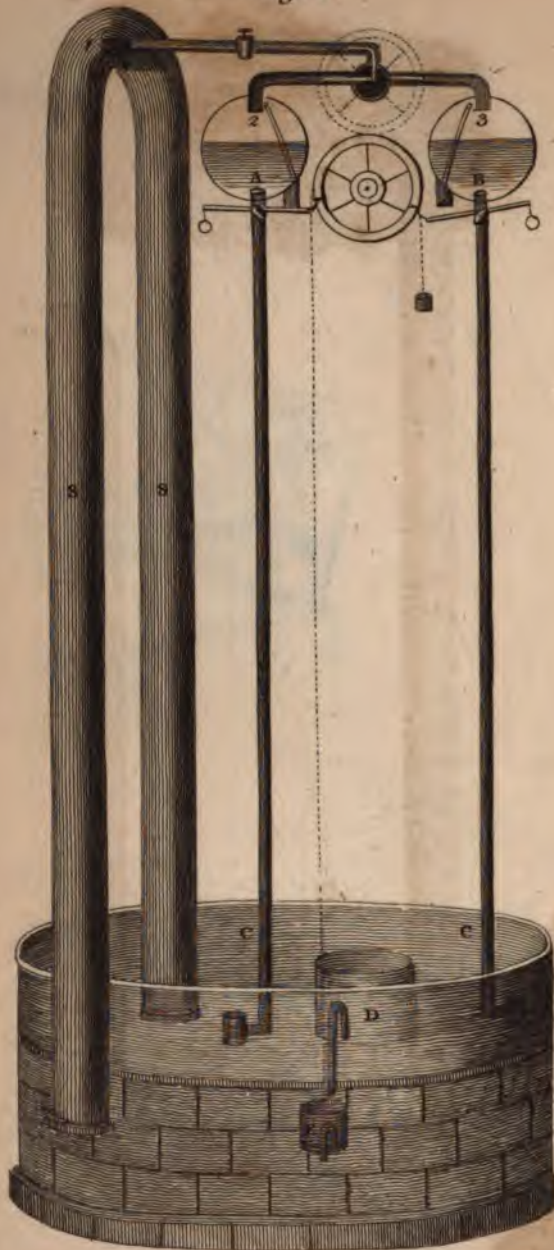




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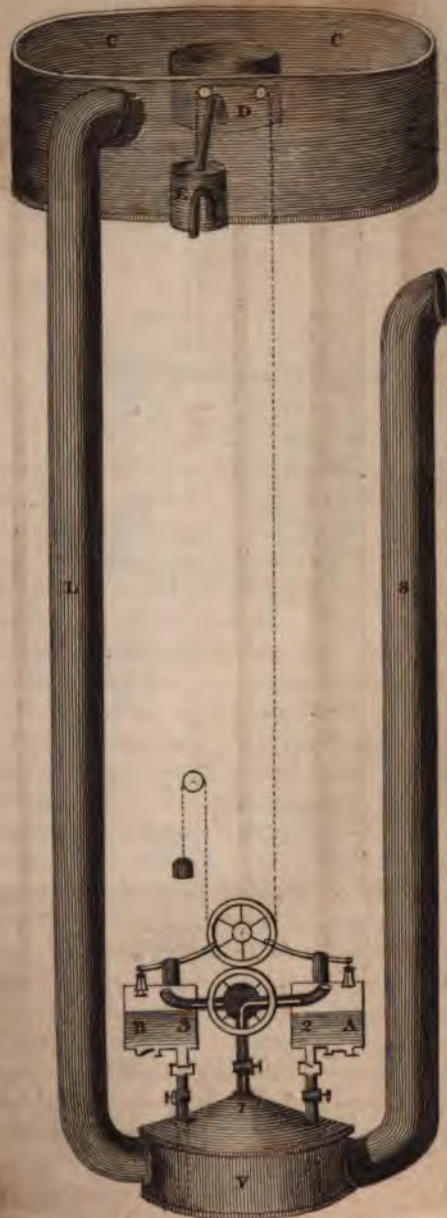
Close's hydraulic apparatus.

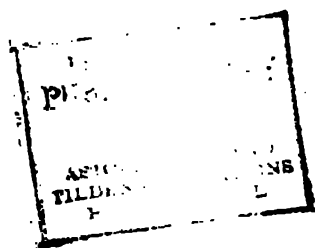
Fig. 1.



Close's hydraulic apparatus.

Fig. 2.





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AND
THE ARTS.

FEBRUARY, 1802.

ARTICLE I.

On the supposed Currents in hot Liquids. By THOMAS THOMSON, M. D. Lecturer on Chemistry in Edinburgh. (From the Author.)

THERE is perhaps no subject of enquiry in which we are more liable to deceive ourselves than in the investigation of the internal motions of the particles of bodies. As these particles are individually too minute to come under the cognifance of our senses, their motions must of course be invisible, and can only be inferred from certain visible changes which we consider as the consequences, or at least as the constant attendants of these motions. But unfortunately it seldom happens that the phenomena, from which we draw our inferences, are of such a nature as to entitle us to affirm that they can only originate from one single cause, and from no other. They may be assigned in general to a variety of causes; and much patient industry, with no small share of address, is necessary to develop the true one; and even all the precautions which we can employ do not always prevent us from falling into error. The phenomenon, which forms the subject of this paper, affords a very striking illustration of the great caution with which we ought to draw conclusions, even when the inference seems at first sight to follow irresistibly from the phenomenon.

Great difficulty of investigating the internal motions of the particles of bodies.

Concise narrative
of Count Rum-
ford's experi-
ments to render
currents observ-
able in fluids;
by floating grains
of amber.

Count Rumford was first led to suspect that fluids are non-conductors of caloric, by the appearance of certain opaque particles moving upwards and downwards in them, while they were cooling in a window, exposed to the direct light of the sun. He very naturally concluded that these motions were occasioned by the currents of the liquid moving in the same direction with the opaque bodies. Hence he inferred that every individual particle of the liquid was cooled only by depositing its caloric at the surface or the sides of the vessel, and that therefore these particles were incapable of giving out heat to each other. The contrivance by which he rendered these motions visible to the naked eye was in the highest degree simple and ingenious. He mixed amber, in the state of small grains, with an alkaline solution, diluted with water till its specific gravity was exactly the same with that of amber. The amber, of course, remained suspended in any part of the liquid; but as soon as heat was applied, it began to move upwards and downwards, exhibiting the currents to the eye of the spectator.

The motions of
these grains have
always been
thought demon-
strative of the
existence of cur-
rents;

Nothing can appear a more satisfactory proof of the existence of currents in the liquid than the motion of the amber; so convincing is it indeed, that hitherto no person, as far as I know, has ever suspected the possibility of this motion originating from any other cause. I have repeated the experiment frequently, and even exhibited it to others; and though the motions did not exactly correspond with those indicated by Count Rumford, they never failed to produce the fullest conviction of the existence of the very currents which that philosopher had indicated. Yet the fact is, that the motion of the amber is not occasioned by currents in the liquid, and very often exists when there are really no currents at all.* This assertion will probably, at first sight, appear rather extraordinary; but the proofs on which it is founded leave no room for doubt.

but they are not
moved by cur-
rents.

Difficulties at-
tending the sup-
position of cur-
rents as rapid as
the motions of
the amber.

When we reflect upon these supposed currents, several circumstances cannot fail to present themselves, which do not admit of an easy explanation. The motion of the amber is pretty rapid, yet the mere difference in the specific gravity of

* I neglect the trifling currents occasioned by the amber itself, which it would be extremely difficult to appreciate.

the hot and cold particles of the liquid, the causes assigned by Count Rumford for the existence of the supposed currents, is capable of producing only a motion so very slow, as scarcely to be visible. But what is still more puzzling, if possible, is the total discordance between these rapid currents and the slow rate at which the liquid cools. If the water actually moved with the rapidity indicated by the amber, and if it parted with the tenth part of the caloric necessary to produce that rapidity, the whole of the liquid would cool down to the temperature of the atmosphere almost in an instant: whereas, in fact, several hours are always necessary to produce that effect.

These apparent anomalies suggested the possibility of the Experiment 1. motion of the amber being occasioned by some other cause. Upon water, tinged blue, was than currents in the liquid, and made it necessary to examine poured clear the matter with more attention. The following experiments water with such care as not to occurred to me as the simplest method of determining whether mix them. Several opaque bodies floated in these fluids. Into the glass vessel A, Pl. VIII. Fig. 2, I poured water, tinged blue with the juice of red cabbage, till it stood at the height *m*. Then by means of a slip of cork swimming on the surface of the blue infusion, and a funnel terminating in a long capillary tube, I succeeded (after several unsuccessful attempts) in filling the vessel with water to the height *n*, without mixing it with the blue infusion. Thus I had two liquids in the vessel A, both very nearly of the same specific gravity, the undermost of a blue colour, the uppermost colourless, and the plane *m*, which separated them, pretty well defined. There were several opaque bodies floating through the blue infusion, which answered the purpose of the amber.

My intention was to apply heat to the bottom of the vessel A, which I knew would produce the supposed currents, that it was expected, when heat should be applied, either is to say, would set the floating bodies in motion. Now if that the lower fluid would ascend in a blue current and carry these bodies up the opaque bodies, or that these bodies would ascend alone through both fluids, and consequently without the mediation of any current. these floating bodies were really put in motion by currents, it is evident that part of the blue infusion must accompany them. But this would become evident whenever they passed the plane *m*, and entered into the colourless medium: for in that case they would be accompanied by a blue current, which would be visible to the eye. Whereas if the floating bodies were in motion while the water continued at rest, they would pass the plane *m*, and move through the colourless water without current.

without carrying any of the blue infusion along with them. On trial the bodies rose and circulated through both fluids without any indication of a current;

and ten minutes elapsed before the motion had carried the blue tinge through the upper fluid. Experiment 2: An alkaline solution, with floating grains of amber, was boiled, and left to cool to 150°. The upper part, to the depth of half an inch, was then tinged. No change whatever took place in the tinged fluid, though the amber circulated through the whole fluid, while cooling.

I accordingly lighted a lamp, and put it at some distance below the vessel A, placing the vessel exactly between my eye and the window. The lamp was at such a distance, that it was some time before the floating bodies began to move. At last, however, they ascended gradually, passed the plane *m*, and entered the colourless medium, *without being accompanied by any of the blue infusion*. They then descended again in the usual manner; and this alternate motion continued for some time, without occasioning a mixture of the two liquids. Thus I obtained a full demonstration that the motion of the floating bodies was not occasioned by currents in the liquid, but was altogether independent of any such currents. Every time that a floating body passed the line *m*, the blue infusion was agitated, and its surface raised into a wave. By the constant repetition of this undulation, the plane *m* became ill-defined, and gradually ascended higher and higher, till at last almost the whole of the water acquired a blue tinge. But it was almost ten minutes before this happened, and the motion of the floating particles, which continued without interruption during the whole time, was altogether unequivocal.

It was still necessary to repeat the experiment with the amber itself, in order to ascertain whether or not its motions were of the same nature with that of the floating bodies in the last experiment. I therefore filled the glass vessel A, to the height *n*, with an alkaline solution containing amber floating in it. I then heated the solution boiling hot by holding the vessel over a fire, and even allowed it to boil for some time. The vessel was then suspended in a room, where the temperature of the air was 50°, and the alkaline solution allowed to cool down to 150°. * I then tinged the surface of the solution by means of a drop or two of the infusion of red cabbage, so as to form a coloured stratum of about half an inch thick at the surface of the solution. † The cooling continued without interruption, and the liquid reached the temperature of the air without the smallest alteration in the coloured stratum, or any mixture of it with the colourless

* By previous trials I had ascertained that at a higher temperature the experiment does not succeed.

† The colour thus communicated is yellow when only a little cabbage juice is used; too much gives a green colour.

solution

solution below. Yet, during the first part of the cooling, the amber was moving upwards and downwards; particles of it often come to the very surface, and descended again to the bottom. This experiment proves decisively that the motion of the amber is not occasioned by currents, and that there were, in reality, no currents at all during the whole time of cooling from 150° to 50° .

Having thus proved that the motion of the amber is not owing to currents, but that it forces its way through the liquid; it becomes a point of some consequence to ascertain, if possible, the cause of this motion. Perhaps the following observations will contribute to throw some light on the subject.

1. When an alkaline solution containing amber, as exactly as possible of the same specific gravity with itself, is heated in a glass vessel nearly to the boiling temperature, and then suspended in the air to cool, the whole of the amber sinks to the bottom. Hence we see that, though the amber is of the same specific gravity with the cold solution, it is specifically heavier than the same solution when hot. Consequently amber does not expand so much when heated as the alkaline solution does.

Why does the amber thus move through the fluid?

Observation 1. Amber expands less by boiling heat than the alkaline solution.

2. But the amber does not remain long at the bottom; the grains of it gradually rise one after another, and ascend to the surface, but with very different velocities, some rapidly, others slowly. After touching the surface they always fall down again to the bottom. These alternate motions continue for some time; but the number of ascending and descending particles gradually diminishes, and at last they all settle at the bottom. It is no uncommon thing for two particles of amber, one ascending, the other descending, to meet together in some part of the solution. In that case they sometimes exchange motions; the one that was formerly ascending now sinking to the bottom, while the descending particle returns again to the surface. If the experiment be made with a transparent solution, and in a good light, every ascending particle of amber will be observed to have an air bubble attached to it which buoys it up. This bubble separates at the surface of the liquid, and the amber, thus deprived of its buoy, tumbles down again to the bottom. When two particles meet, the air bubble sometimes passes from the one to the

Obs. 2. Each particle of amber is carried up by an adherent air-bubble, which it loses at the surface, and then falls again.

the other, which occasions their reciprocal change of motion. These air bubbles are of different sizes, hence the different velocities of the pieces of amber.

Obs. 3. The amber acquires a higher temperature by boiling than the solution itself, and consequently it generates steam.

3. If the alkaline solution be made to boil for a few minutes in a glass vessel, and then suspended immediately in the air to cool, bubbles of steam continue for some time to detach themselves in great abundance from the pieces of amber, and occasion a very brisk motion in these pieces. Hence we see that the amber acquires a temperature higher than 212° , and that it gradually parts with this excess of caloric to the surrounding liquid.

Obs. 4. Irregular motions after the steam has ceased; followed by subsidence.

4. After all extrication of steam ceases, the amber continues to move rapidly upwards and downwards in a very irregular manner. The rapidity of these motions gradually diminishes, and when the solution has cooled down to about 140° (the air being 50°) the amber settles at the bottom. * These motions are too rapid to be occasioned by the cooling of the liquid, yet they are certainly connected with its high temperature. I would have supposed that they indicated the currents into which the liquid was thrown by boiling, as they are extremely similar to what takes place when the liquid is agitated violently, were it not that they last much longer, and that they always stop when the difference between the temperature of the liquid and of the air is about 90° .

Obs. 5. Ascent of the amber; whence it is inferred that it cools less rapidly than the fluid.

5. After the amber settles at the bottom, the cooling goes on without any thing remarkable till the liquid reaches the temperature of about 100° (the air being at 50°). At that temperature some of the amber may be observed suspended just above the bottom of the vessel; and, as the cooling advances, the amber gradually rises higher and higher. No motion can indeed be perceived, but if the situation of any individual particle be observed at intervals, it will appear evident that it has changed its place, and ascended nearer the

* This experiment requires that the amber be in the state of a pretty fine sand, otherwise it settles at the bottom at the very first. But even then the currents may be distinguished by the naked eye, if the glass vessel be transparent. This fact is curious, and can only be explained by supposing a greater interval than usual between the currents and the rest of the water, or by supposing their specific gravity to be different.

surface.

surface. By the time the liquid has reached the temperature of the air, some of the amber may be observed floating on its surface, some of it a little below the surface, and, in short, in almost every part of the liquid. After this the liquid may be left at rest as long as you please; but the amber does not change its situation, unless some very considerable change takes place in the temperature of the surrounding air. Here we see that the water cools down more rapidly than the amber; so that at a certain period the amber becomes specifically lighter than the water, and therefore rises; extremely slowly indeed, because the difference between the specific gravity of the two bodies is small. I supposed at first that the rise of the amber was owing to the descent of the superior strata of the liquid, which, of course, would have forced up the strata in which the amber was; but I was mistaken. For when the surface of the solution was tinged yellow, the yellow stratum remained neatly defined, and without changing its place during the whole of the process.

This ascent is not caused by current.

Annotation. Atwood, in his Treatise on Motion, Sect. V. Ascent of a sphere in a fluid, Prop. 12, demonstrates that a sphere of evanescent weight will ascend in any dense fluid uniformly with a velocity equal to what it would have acquired by falling in free space through four third parts of its diameter. This is the greatest possible velocity of ascent of a sphere in any fluid. It may seem absurd to attempt a calculation, one of the assumptions of which shall be the diameter of an integrant particle; but if we take any gross quantity (such, for example, as the millionth part of an inch), for the diameter of a particle of water at its utmost expansion by heat, and deduce its velocity of ascent (which in that case could scarce amount to the two hundredth part of an inch in a minute), we shall find little reason to admit that ascending currents can be produced by the mere enlargement of the particles heated only by contact with the containing vessel. This, however, must necessarily be supposed if the particles be non-conductors with regard to each other.

Motion of the particles of water by mere expansion must be extremely slow

The excellent paper before us directs our attention to the mechanical cause of that circulation, which undoubtedly takes place during the communication of heat to fluids from below,

Circulation of fluids by change of temperature

and is the principal reason of their much speedier augmentation of temperature in this way. The extrication and ascent of elastic fluid appears, in most and probably in all cases, to precede, and at last to produce circulation in dense fluids. And on this object there is still an extensive field for research, concerning the manner in which heat is communicated through fluids. But it would be by no means fair to anticipate, by crude conjecture, those explanations which we may expect to obtain from the experimental research of others. W. N.

II.

A Memoir on the Art of making Gun-Flints. By CITIZEN DOLOMIEU.*

No information
to be found in
books on the art
of making gun-
flints.

THE art of making gun-flints, which has been long practised in a small territory situated between the two neighbouring departments of Loir-Cher and L'Indre, is directed to an object, of which the whole value, commercially considered, is of little magnitude; but which is of the first necessity in the use of fire-arms of every description. The author of the present Memoir was desirous of acquiring some information respecting it, but found that it was to no purpose that he extended his researches to a variety of works on mineralogy and different subjects of the arts and trade. The *Encyclopédie* is silent on this process; excepting in the repetition of a ridiculous prejudice concerning the re-production of flints in places whence they were excavated. From the precision with which the figure is given to these stones, it has even been suspected that they could hardly be afforded at the low price they bear, if in its first state the material were not even soft. But the art is extremely simple in its process; it requires a very small number of instruments, a short apprenticeship, and a very moderate degree of skill to form, by mere fracture, figures so accurate, faces so smooth, outlines so direct, and angles so neat, that the stone seems as if cut on the wheel of the lapidary. Five or six small blows of the hammer, during one minute of time, are sufficient to produce

* From *Memoires de l'Institut Nationale*, III. 348, in part abridged.

the

the same perfection of figure as would require more than an hour's labour, if the sections were to be made by grinding against harder substances, or friction with emery. Less than a farthing will pay for a gun-flint from the hand of the workman, but fifty times that sum would be insufficient for its purchase if it were fashioned by any other process.

The author regularly proceeds to examine first the material best adapted to the use in question, the instruments employed, and the manipulation by which the stones are fashioned.

● With regard to the material, every kind of stone, capable of producing strong sparks when struck against steel, may be used as a gun-flint, if it can but be fashioned by simple and cheap means. But even in this case there are some motives of preference; such, for example, as that the scintillation should be produced with the least possible blow, and with no considerable wear or abration of the steel. These reasons of predilection are in favour of the siliceous stones, when compared with those which are called quartzose. But the flint or flint, properly so called, possesses not only this kind of superiority, but another property, that it is more particularly susceptible of being broken into fragments or plates, which require but very little labour to give them the requisite form and dimensions.

Among the flint it is therefore that the fabrication of gun-flints have found the material truly proper for the exercise of the art. And among the numerous varieties of this species of stone, there is only one which can be advantageously fashioned by the hammer alone. The agates and chalcedonies, which are also applied to this use, are brought to the requisite form by the mill of the lapidary. The makers of gun-flints in France denominate the stone of which they make use *caillou*, and they themselves are called *caillouteurs*. The word *caillou* is used by them to denote the best and most serviceable kind of flint; whereas, in the other parts of France, it denotes a pebble; that is to say, a rounded stone, whatever may be its nature.

The flint of the workmen in gun-flints belongs to that species of flint which naturalists have denominated flint *grecus*, flint *ignarius*, or the *feuerstein* of the Germans, &c. But every coarse flint is not proper for this use. In fact, the best stone is far from being plentiful in nature. Many countries

tries are entirely deprived of them; and the author thinks that it may probably be affirmed, that France almost alone possesses that variety of *filex* which can be easily broken into gun-flints, since he cannot suppose that the art of making them could remain a mystery to other nations who do not practice it, though they make great use of the flints: the art itself being so simple, that they must have speedily acquired it, if they have possessed the material.

Dissimination. In his description of the variety of *filex* here alluded to, he gives it the name of *filex pyromachus* to express its use, which he prefers to the term *filex sclopetarius*, as being more musical.

The external characters are :

External characters. The *filex pyromachus*, when dug up, is always covered with a white external crust, one or two lines or more in thickness, of an earthy, chalky appearance, and loose texture, much softer and less heavy than the *filex* it envelopes. The external form of the masses of good stones of this description has a somewhat convex surface, approaching to the globular figure. Those of irregular forms are full of imperfections. The best stones are not very large. They seldom exceed the weight of twenty pounds, and they ought not to be of less weight than one or two pounds. Their aspect, when broken, is greasy, shining a little, and the grain is so fine, that it is imperceptible. The colour of these good stones may vary from the yellow colour of honey to a blackish brown. In this respect it is to be noted that the value of a stone does not depend on its colour, but on the uniformity of the tint, which becomes less intense when the stone is reduced into thin splinters. The flints of the two departments first mentioned are yellowish. Those of the chalk hills on the banks of the Seine are blackish brown. Both the one and the other, when pulverised, are perfectly white. The *filex pyromachus* ought to possess an uniform semi-transparency, of a greasy aspect, to such a degree, as to admit letters to be distinguished through a piece of the stone of one-fiftieth of an inch thick, laid close upon the paper. Its fracture must be smooth and equal throughout, and very slightly conchoidal; that is to say, convex or concave. This kind of fracture is one of the most essential properties upon which the faculty of being divided into gun-flints depends,

The

The workmen select the stones proper for their use by their external character. They compare that part of the masses of filex, which is uniformly semi-transparent and coarsely loaded, to the inner skin of bacon, which they call *couenne*. They say that one flint has more or less *couenne*, or is more or less fat than another. They assert that the upper part of a flint is always of a better quality than the lower. Flints are considered as imperfect or intractable when naturally deprived of any of the external characters before indicated, or when injured by long exposure to the air. Most of the masses are subject to have whitish opaque spots and a kind of knots where the material is harder than in the other part of the stone. When these accidents are too abundant, the stone is rejected as useless.

The physical characters are as follow :—Specific gravity of the white filex pyromachus from the banks of the Cher proved to be 26041; that of the blackish kind from the chalk hills of Laroche Guion was 25954. In this respect it does not differ essentially from the other varieties of filex, of which the specific gravities are usually between 26100 and 25900. Hardness a little superior to that of jasper, but inferior to that of agate and chalcedony. It is nearly the same as that of the other common flints. Brittleness. It is more brittle than most other siliceous stones. The light coloured is more so than the darker; these last being rather more scintillant, and wear away the hammer more quickly. When two pieces of the filex pyromachus are strongly rubbed together, they emit the peculiar well known smell of siliceous stones, but in this variety it is more strong than in any other.

Chemical characters. Action of the air.

The filex pyromachus, deprived of its natural coating, and exposed for a long time to the changes of the atmosphere, acquires a second white friable coating, consisting of the filex reduced to powder; and even its internal part loses its greasy appearance and semi-transparency, so as to become whitish. In this case the specific gravity, which was 25954, becomes 25754, consequently it has lost 200 parts of the weight it possessed at first.

The filex pyromachus is sometimes too moist when taken out of the quarry. It requires to be dried; but if by too long exposure to the air or the wind it loses a certain portion of its humidity.

humidity which is often very perceptible when it is recent, it can then no longer be broken into gun-flints, as its fracture is less easy. The workmen carefully reject all those which have lost this favorable degree of moisture. Perhaps they might be restored by keeping them in a damp place, or covering them with earth, and by these means at least they might succeed in preserving those intended to be worked up in winter.

**Effect of heat.
Oxidation.**

When the fragments are thrown upon a red hot plate of iron it flies and cracks, and becomes opaque. When projected in powder upon nitre in fusion, it gives a few sparks with slight inflammation and detonation.

When calcined in a test it loses one 250th part of its weight, increases in bulk, becomes extremely white, and so brittle as to be almost friable. In this state it resembles the finest porcelain biscuit.

Distillation.

When distilled in a retort by strong heat it affords a little carbonic acid gas, and a quantity of water amounting to 200 parts of the weight before indicated as its specific gravity; but gives no sign of the combustible matter which in the preceding experiment caused the nitre to detonate.

**The flint is an
imperfect hy-
drophanes.**

This water, which appears essential to all the flints, and may be called their radical water, is the cause of their transparency. Exposure to air by drying them renders them opaque; so that they may be considered as imperfect hydrophanes; for they do not again absorb, but with difficulty the water necessary to their transparency. This water also contributes to the connection of their integrant particles, whence their fracture becomes more equal, and is harsh when they have lost it. These flints when recently dug up even afford an aqueous vapour when struck, and the face of the fracture is humid, and as it were moist.

Chemical analysis.

**Chemical analysis;
fusion with
potash; precipi-
tation of the
silica, &c.**

Citizen Vauquelin examined 100 parts of *silica pyromachus* of a brownish colour, and uniformly semi-transparent from the hills of La Roche Guion. He mixed the mass with 400 grains of very pure potash, which by fusion in a silver crucible afforded a compound, which after cooling was diffused in water, and then super-saturated with muriatic acid. The very clear solution was evaporated to dryness, re-dissolved in water, and the silica thus saturated, and left upon the filter after being well washed,

washed, dried and ignited, weighed 97 grains. Ammonia was afterwards added to the clear liquid, where it produced a slight yellowish white precipitate, which after being well washed and dried weighed one grain, and was found to be a mixture of alumine and oxide of iron. The fluid separated from this small portion of iron and alumine, gave no other precipitate on the addition of carbonate of potash, and the waters used in the washing left no residue when they were evaporated to dryness. The result therefore was filex = 97 grains, alumine and oxide of iron = 1, loss = 2. The author considers it to be a very remarkable fact, that the filex pyromachus should contain only filex and water, the alumine and iron being too small in quantity to be considered as essential to its composition, or to influence its habitudes. Quartz also, from the analyses which have been made, appears to contain only filex; yet the more he examines the two substances, the more he finds reason to suppose them essentially different from each other; since the one refuses to assume the crystalline form, and the other assumes it and becomes clear, even in contact with the flint itself. The one appears to be incapable of admitting water into its composition, while the other constantly contains it until it begins to be decomposed. He offers a query, whether the difference may not consist in the small portion of combustible or fatty matter, which caused the detonation with nitre, or whether the quartz may not, like alum, acquire its property of crystallizing from the addition of some other substance. This question, as he remarks, must be resolved by future experimental enquiries.

Difference between filex and quartz.

The analysis of the whitish spots afforded filex 98 grains, oxide of iron 1, carbonate of lime 2. That of the absolutely opaque parts gave five parts more of carbonate of lime; and lastly, the analysis of the white coating naturally enveloping the masses, afforded 86 parts of filex, 1 oxide of iron, 10 carbonate of lime, and 3 loss. Those analyses which afforded no alumine, shew that this earth is not essential to the filex, and the absence of lime in the first analysis shews that it was an accidental ingredient in the latter.

Mineralogical situation. In France in the environs of St. Aignan, situated in the department of Loir-Cher, and in that of L'Indre, and the departments which occupy the vallies of Siene and Marne, are principally the places where this stone

Mineralogical situation.

stone is found. It exists in the chalky calcareous stones, in chalks more or less fine and solid, and in marles. They form horizontal strata, by the manner in which the large and small masses are placed beside each other. Nevertheless, as the blocks of flint do not accurately touch each other, there is no solution of continuity between the upper and lower masses of chalk.

Out of twenty beds of flint lying one above the other, at a distance of twenty feet or less, there will frequently be no more than one, and very seldom two, which afford good stones of this description; but in the bed which affords them, almost all the blocks have a greasy appearance, and in the other strata scarcely any of this description will be found. Accordingly the good strata are followed by subterraneous excavations, frequently at considerable expence, while the others are neglected.

On the banks of the Cher the flints are explored in a plain, by digging shafts to the depth of 40 or 50 feet, from whence horizontal galleries are carried into the only good stratum which is known.

On the banks of the Seine in the hills of La Roche Guion, the cliffs of chalk present steep precipices, where the strata of flint are exposed; and one of these strata, which contains good stones for gun-flints, is about six toises from the upper surface of the great mass of chalk.

Instruments.

Description of
tools or instru-
ments.

The mace.

The instruments used for fashioning the gun-flints are four in number:

1. A small piece of iron or mace, with a square head. Plate V. Fig. 1. the weight of which does not exceed two pounds, or perhaps a pound and a half, with a handle seven or eight inches long. This instrument is not made of steel, because if it were too hard, its stroke might shatter the flint, instead of breaking it by a clear fracture.

The hammer.

2. A hammer with two points, in which the position of the points is of consequence as to the nature of the stroke, Fig. 2. This hammer, which must be of good steel well hardened, and does not weigh more than sixteen ounces; some do not exceed ten. It is fixed on a handle seven inches long, which passes through it in such a manner, that the points of the hammer are nearer the hand of the workman, than the center of gravity

gravity of the mafs. The form and fize of the hammers of different workmen vary a little, but this difpofition of the points is common to them all, and is of confequence to the force and certainty of the blow.

3. A little inftrumen tnamed Roulette (roller) which repre- Roulette.
fents a folid wheel, or fegment of a cylinder, two inches and one third in diameter. Its weight does not exceed twelve ounces, it is made of fteel not hardened, and is fixed on a fmall handle fix inches long, which paffes through a fquare hole in its center.

4. A chiffel bevelled on both fides, feven or eight inches Chiffel.
long, and two inches wide, of fteel not hardened; it is fet on the block of wood which ferves as a work bench, out of which it rifes to the height of four or five inches. To thefe four inftruments we may add a file, for the purpofe of reftoring the edge of the chiffel from time to time.

The procefs :

After felecting a good mafs of flint, the whole operation Manipulations.
may be divided into four manipulations.

1. To break the block. The workman being feated on the Break the block.
ground, places the flint on his left thigh, and ftrikes it gently with the larger hammer, Fig. 1. to divide it into portions according to its fize, that is to fay, of about a pound and a half each, with broad furfaces nearly flat. He is careful not to crack or produce flakes in the flint by ftriking it too hard.

2. To cleave the flint, or break it into fcales. The prin- Cleave or fcale
ciple operation of this art is to cleave the flint well: that is to the flint.
fay, to feparate from it pieces of the length, thicknefs, and figure, adapted to be afterwards fashioned into gun-flints; and in this part the greateft degree of addrefs, and certainty of manipulation are required. The ftone has no particular di- No particular
rection in which it can be moft eafily broken. The courfe of direction of
its fracture depends entirely upon the choice of the workman. fracture.
In this procefs he holds a piece of flint in his left hand, not fupported, and ftrikes with the hammer, Fig. 2. on the broad faces produced by the firft fracture, in fuch a manner as to chip off the white coating of the ftone in fmall fcales, and to lay bare the flint in the manner represented, Fig. 5.; after which he continues to ftrike off other fimilar portions of the pure flint. Thefe pieces are nearly an inch and a half wide, two inches and a half long, and one fixth of an inch thick in the middle.

They

**Figure of the
scales.**

They are slightly convex within, and consequently leave a space somewhat concave, terminating longitudinally in two lines, somewhat projecting, and nearly strait. The prominent edges produced by the fracture of the first scales, must afterwards constitute nearly the middle of the subsequent pieces; and those pieces only, in which they are found, can be used to form gun-flints.

In this manner the operator continues to cleave, or scale the stone in different directions, until the natural defects of the mass render it impossible to make the fractures required, or until the piece is reduced too much to receive the small blows which separate the pieces.

3. To fashion the flint.

**Fashion or form
the flint.**

The gun-flint, Fig. 7. may be distinguished into five parts, namely, 1. the edge, or bevel part, which strikes the hammer or steel. This is two or three twelfths of an inch in width. If it were broader it would be too liable to break, and if more obtuse it would not afford a brisk fire. 2dly. The side edges, which are always somewhat irregular. 3dly. The back edge, most remote from the hammer where the stone possesses its intire thickness. 4thly. The under surface, which is smooth and slightly convex. And 5thly. The upper face, which is slightly concave, and receives the action of the upper claw of the cock, in which it is fixed for service.

Operation.

In order to fashion the stone, those scales or chips are selected, which have at least one longitudinal prominent angle. One of the two edges is fixed on to form the striking edge; after which the two sides of the stone which are to form the side edges, and that which is to form the hinder edge are successively placed with the convex surface upon the edge of the chissel, which is supported with the fore-finger of the left hand, at the same time that a small blow or two is given above the point of support with the Roulette, Fig. 3. by which the stone breaks exactly along the edge of the chissel, as if it had been cut. In this manner the sides and posterior edge of the stone are made.

**Complete the
edge**

4. The stone being thus reduced to its proper figure, the finishing operation consists in completing its edge in a strait line. For this purpose the stone is turned, and the under flat part of the edge is placed on the chissel, in which situation it is completed by five or six small strokes with the Roulette.

The

The whole operation of fashioning a gun-flint is performed in less than one minute.

A good workman can prepare a thousand good chips or scales in a day, if his flints be of good quality, and he can also fashion five hundred gun-flints in a day; consequently in three days he will cleave and finish a thousand gun-flints without further assistance. The rate of working. On thousand flints in three days.

This manufacture leaves a great quantity of refuse; that is to say, about three fourths of the whole stone: For there are not more than half the scales which prove to be well figured, and nearly half the mass in the best flints is incapable of being chipped out: so that it seldom happens that the largest piece will afford more than fifty gun-flints. The larger pieces of refuse are sold for the culinary purpose of striking a light. Much refuse or waste.

The gun-flints when completed are sorted out, and sold at different prices, according to their degrees of perfection, from 4 to 6 decimes (or pence) the hundred. They are classed into fine flints and common flints; and according to their application into flints for pistols, fowling pieces, and muskets.

The fabrication and commerce of gun-flints in France is in some measure confined to three communes of the department of Loir-et-Cher, and one department of the Indre, as was before mentioned, namely, the commune of Noyers, 2,400 metres east north-east of St. Aignan; the commune of Couffy at 5,600 metres, and that of Meunes at one miriameter east south-east, and in the latter department, the commune of Lye 9 kilometers to the south-west of St. Aignan. The inhabitants of these communes employed in this species of industry are about 800, and they have excavated great part of the plain they inhabit. Local situation of the manufactories in France

A single workman named Stephen Buffet, who emigrated from the commune of Meunes to the banks of the Seine, where he has carried on this art for about thirty years by himself, was the person from whom Dolomieu obtained the present instructions. There are a few other places in France where this art is practised, but in none to the extent of the places before mentioned. The author has not met with this manufactory in any other countries, except in the territory of Vicenza, and one of the cantons of Sicily. He remarks that it may be carried on elsewhere, though probably overlooked by travellers, on account of its apparent insignificance. I believe it is practised at Purfleet, in the county of Kent, and in various other parts of England. Elsewhere practised.

III.

Letter from Dr. BEDDOES on Prognostics of the Weather, the Effects of the Nitrous Oxide, and other Objects.

To Mr. NICHOLSON.

SIR,

Clifton, Jan. 9, 1802.

Probability that the ensuing spring and summer may prove cold.

SINCE the late fall of snow, I have been asked by several people, how I would now apply the principle, stated in a letter, inserted in one of your former Numbers *. My answer was, that it induced me to suspect a corresponding great fall in the N. and N. E. regions. If this be the fact, and if no unknown calorific process take place between us and the equator, it would furnish an unfavourable prognostic with regard to the weather of the ensuing spring and summer.

How the weather is to be foreknown.

We can, I suspect, only attain a degree of prescience concerning the seasons, by comparing the order of the most obvious meteorological events. This, you know, was the way in which the first improvements were made in astronomy. In order to gain another point of comparison, I beg leave diffidently to propose a conjecture, respecting the duration of different falls of snow. The duration, I suppose, will bear some proportion to the quantity on the ground. For example, *if an inch of snow fall, when the thermometer has been within the twelve preceding hours above 32° it will either lie a much shorter time than if six inches fall, so as to lie under the same condition.* Perhaps the duration will generally be at least equal to the squares of the depths.

Conjecture.

Atmospheric currents of different temperature.

You will suppose that I found my conjecture upon some sort of reasoning. I deduced falls both of snow and rain, after Dr. Hutton, from the mixture of airs, unequally heated. I consider snow falling and lying in quantity, as the sign of a great current of air from the polar regions, particularly when the wind has blown for some time from the opposite points;

* Philof. Journal, Quarto V. 131. The principle is that, *If there be an unusual fall of snow in the countries on the N. and N. E. our summer, ceteris paribus, will be cold and wet.*

that

that is, when a large body of air, at above 32° , has gone to the northward of these islands. If the snow lie to any depth, I imagine this to be a sign of the preponderance of an under-current from the same quarter, which will generally produce a frost of some continuance, as well as snow in the first instance. Where a S. or S. W. wind prevails, the snow will change into rain, and not lie to any depth.

I abridge this precarious speculation, (which it would be *Query*. easy to extend into a pamphlet) and suppress many explanations, in order merely to propose, as a query, *Whether a snow, like that which now covers the earth, be not always actually the forerunner of a frost of some days, and generally of some weeks, continuance?*

During the present week we had the curious phenomenon On the intermediate thaw. of a thaw for (I believe) near 48 hours, with the wind at N. E. which no doubt arose from a considerable upper current from the contrary quarter, at no great elevation. But the quantity of snow, already on the ground, made me venture repeatedly to say, that the weather would not soon change to a complete thaw.

In a short time I intend to put to press a report of the operations at the pneumatic institution here. The nitrous oxide Medical effects of the nitrous oxide, &c. has justified the expectation, originally formed of its powers in palsy, and also as a *restorative*, if I may employ this abused word, to certain constitutions uniformly broken down, and has proved an admirable *soother* of the sufferings of old age. We (I speak of myself and other persons, concerned at the institution) have frequently witnessed in new patients the temporary restitution of the voluntary power over a perfectly paralytic limb during the act of inhalation. This power has become permanent afterwards in some instances, but not always, though perhaps not from want of efficacy in the air. It has also been discovered here, that oxygen gas, long inhaled, affects the growth and the internal parts, especially the bones, in a very extraordinary manner. The effect on the bones I had anticipated, and the altered growth, seems a natural consequence. Some accurate drawings by Mr. King will serve to exhibit these effects.

NUTRIMENT OBTAINED FROM BONES.

'We shall have to report also some considerable improvements in medicine from the use of new, or little known remedies. Will you allow me to mention, that I should be glad to engage with an active and ardent young enquirer, who has already some readiness in common chemical processes. His employment would be in researches, connected with medicine and physiology. If such a person, resident in London, should be induced by this notice to inquire, he might obtain some previous satisfactory information from Mr. Davy at the Royal Institution.

Wishing your improved Journal the support it merits,

I am, SIR,

Respectfully your's,

THOMAS BEDDOES.

IV.

On the Quantity of Nutriment to be obtained from various Kinds of Bones, and the best Method of extracting it, by Professor PROUST.

Proust on improving the subsistence of the soldier. MR. PROUST, professor of chemistry at Madrid, has published a work, intitled an Inquiry into the Means of Improving the Subsistence of the Soldier. The following is a summary of what it contains of most importance, and of general utility.

Jelly from bones. It has long been known, that a very agreeable and nutritious food might be procured from bones: a food particularly recommended as a restorative under the name of hartshorn jelly, which is in fact procured by boiling shavings of bones, the common hartshorn shavings of the shops. All that is wanting, is a cheap and easy mode of procuring this jelly in sufficient quantity from a substance now commonly thrown away; for bones after they come out of the pot are as good for the purpose, and nearly as productive, as fresh bones. The shavings are too dear. Papin's digester, which has been employed, is a machine too expensive for the poor, too dangerous for common use, and apt to give the jelly a burned taste.

To

To obviate all these objections, nothing more is necessary, ^{Mode of pre-} than to reduce the bones to powder; which may be very rea- ^{curing it.} dily done between a pair of toothed iron cylinders, as in the ammoniac works. The bones thus comminuted are to be boiled in eight or ten times their weight of water for the space of four hours, or till about half the water is wasted, when the liquor will be found on cooling of a due gelatinous consistence. A vessel with a tight cover should be used, that the water may acquire as much heat as possible; and it should not be of copper, as this metal is easily dissolved by animal mucilage.

It is to be observed, that bones from different parts afford ^{Proportions of} different proportions of jelly, by which of course the quantity ^{jelly in differen} of water should be regulated. According to the experiments ^{bones.} of Mr. P. five pounds of the middle part of the bone of a leg of beef will afford nine pints of jelly; the same quantity of the bone of the joint, fifteen pints; of the ribs and spine, eleven quarts; of the rump and edge bone, thirteen quarts. Five pounds of mutton bones of every sort together give nineteen pints of jelly. Pig's bones yield a little more. To Mr. P's taste, the jelly from pig's bones was the most agreeable of all: that from mutton had the distinguishing flavour of the meat. Of the jellies from beef bones that from the ribs was most pleasing, both to the sight and palate, that from the leg and joints least.

In warm weather the liquor must be boiled down somewhat more, if it be intended to assume the same gelatinous consistence when cold; as the same quantity of bone, that would afford a quart of jelly in winter, will not yield above a pint and a half, or a pint and a quarter in summer, but then it contains proportionally more nourishment.

If this jelly be boiled till it acquires a consistence a little ^{Portable jelly} thicker than a syrup, then poured out into plates, and when cold cut into pieces and dried on a net, it will keep a long time, and be particularly useful at sea. One ounce of this dry portable jelly, being soaked in water for a quarter of an hour to soften it, and then boiled, will make from a pint and a quarter to a quart of jelly, according to the season, and equally as good as that which is fresh extracted.

Mr. P. prepares a very pleasant restorative for the sick, as he informs us, by adding an ounce and a half of sugar, and a little salt, to fourteen or fifteen ounces of the jelly, and then ^{making}

making it into an emulsion with twelve sweet and four bitter almonds, and a little orange peel.

Fat obtained
from bones.

Another advantage to be derived from these refuse bones is the fat. Before grinding them to extract the jelly, Mr. P. chops them into pieces about an inch long with a cleaver, throws them into a kettle of boiling water, and lets them boil about a quarter of an hour. The fat obtained in this manner from sixteen pounds of rump and edge bones weighed, when cold, two pounds; and from the same quantity of the bones of the joints he obtained four pounds of solid fat. This, he observes, when fresh, may be used for various culinary purposes; and when it has been kept for some time exposed to the air, it becomes very good tallow for making candles.

V.

*Observations on the Effects which take Place from the Destruction of the Membrana Tympani of the Ear; with an Account of an Operation for the Removal of a particular Species of Deafness. By Mr. ASTLEY COOPER.**

Injury or loss of
the membrana
tympani does not
occasion deaf-
ness,

Various causes
of its loss or in-
jury.

AFTER referring to a former paper, in which he had proved, by facts, that an aperture in the membrana tympani does not diminish the faculty of hearing, and that a complete destruction of the membrane does not occasion a total loss of that sense, Mr. Cooper observes, that there are various causes by which the membrane may be injured or destroyed. Of these the most common is suppuration in the meatus auditorius. In persons of a delicate constitution and irritable habit the ear-wax is liable to be hardened, gradually bringing on deafness, and then exciting inflammation and suppuration. If this be not remedied, not only the membrane lining the meatus, but that of the tympanum also, will be destroyed, the small bones of the tympanum be discharged, and sometimes considerable exfoliations take place. At Fig. 4, Pl. VI. is a view of the membrane of the tympanum lacerated by a blow on the head; probably from the air being driven into the meatus with violence. Children often introduce small stones, pieces of slate pencil, and even pins, into their ears,

* Abridged from the Philosophical Transactions, 1801, p. 435:
in

in extracting which considerable lacerations have been made in the membrana tympani. Fig. 5 shews one torn in attempting to remove a pin.

The membrana tympani may be easily seen in some persons, by directing the rays of the sun, or a condensed light from a common lamp, into the ear: but this is not the case in all; for the meatus differs considerably in different persons, both in its depth and diameter. If the ear be clear of wax, the membrane has a bright tendinous appearance; and an aperture in it appears as a dark spot, which, by the silvery surface of the membrane surrounding it, is rendered distinctly perceptible. If there be an aperture, air also, upon blowing the nose with violence, will be forced with a whistling noise through the ear; the smoke of tobacco may be driven from the mouth through the ear; or water may be injected from the ear into the throat.

How the membrane may be seen, and

its appearance.

Means of discovering an aperture in it.

If the whole of the membrane be destroyed, and three out of four of the small bones of the tympanum be removed, an almost total deafness ensues; but the ear, after a time, begins to recover its powers, and, in the end, regains them, with that degree of imperfection only, which was described in the case of Mr. P——, in Mr. Cooper's former paper. When the membrane of one ear only is destroyed, a greater degree of deafness takes place in that ear, than would happen in either, were the membrane destroyed in both. This probably arises from the disuse, into which the imperfect ear falls. Thus Mr. G——, at an early period of life, lost a great portion of the membrana tympani of the left ear; and as he heard somewhat better with his right ear, he was little in the habit of employing the left, till at length he considered himself almost totally deaf in it. Becoming deaf in the right ear, however, and consequently obliged to employ the left, he found it by no means deprived of its powers.

Effects of the destruction of the membrane.

Fig. 3.

These observations have induced Mr. Cooper to try the effect of an operation, which has proved successful in several instances of one species of deafness,—that which arises from obstruction of the Eustachian tube. Of this obstruction there are several causes. A common cold, affecting the parts contiguous to the orifice of the tube, may prevent the free passage of the air into the tympanum. The deafness thus occasioned is in general temporary; but it may become permanent,

Cure of a species of deafness.

The Eustachian tube may be obstructed from several causes.

manent, if a frequent recurrence of such attacks produce permanent enlargement of the tonsils. The scarlet fever occasions ulcers in the throat, which, in healing, frequently close the Eustachian tubes. A venereal ulcer may have the same effect. The tube has been closed by an extravasation of blood in the tympanum. And in one instance a stricture in the tube rendered the passage extremely difficult. In this case, by forcing air from the mouth into the cavity of the tympanum, and then pressing gently on the ear to expel part of the air from the cavity, an immediate increase of the power of hearing was produced. These are the most common causes of obstruction in the Eustachian tube; and Mr. Cooper has reason to think, from the experience he has already had, that they may all be remedied by puncturing the membrana tympani. In support of this opinion he gives four select cases, one of which is the following:

Case of deafness
caused by puncture
of the mem-
brane of the
tympanum,

Mr. Round, of Colchester, consulted Dr. Baillie respecting his son, Mr. John Round, aged seventeen, who had laboured, from his birth, under such a degree of deafness, as would have incapacitated him from engaging in business. Dr. Baillie, having satisfied himself that there was no nervous defect in the ear, referred him to Mr. Cooper. He found that this gentleman had been born with an imperfect state of the fauces, which rendered him incapable of blowing his nose; that the Eustachian tubes had no openings into the throat, and, therefore, that he was unable to force air from the mouth into the ear. The auditory nerves, however, were perfect; for he could distinctly hear the beating of a watch, if placed between the teeth, or against the side of the head; and he had never perceived any buzzing noise in his ears. Our author therefore advised him to submit to the operation of perforating the membrana tympani; to which he cheerfully consented. The moment this was done, a new world was opened to him; and the confusion produced by the number of sounds, which immediately struck his ear, made him sink upon a chair, almost in a fainting state. From this state he recovered in about two minutes; and, finding that his hearing was completely restored upon the one side, he wished the operation to be performed upon the other; which was immediately done, with the same happy result, and without his experiencing the same confused sensation as before. Near two months after the operation,

operation, Mr. Cooper had the pleasure to receive an assurance from him, that he had suffered no relapse, nor any inconvenience from the openings which had been made, and that his hearing continued perfect.

The operation consists in passing into the ear a canula, of the size of a common probe, in which a trocar is concealed: the canula is to rest upon the membrana tympani, and the trocar is then to be thrust through the membrane. The trocar should be so adjusted, as not to pass more than one-eighth of an inch beyond the canula, to prevent its reaching the opposite side of the cavity of the tympanum. Should it however touch the periosteum of the tympanum, it can be productive of no serious harm. The aperture should be made in the anterior and inferior part of the membrane, under the handle of the malleus, which must not be injured in the operation; and it is therefore necessary, that the operator be acquainted with the exact situation of this bone. Though the membrana tympani is vascular, the vessels are so small, that they bleed but little; and therefore, if much blood be discharged, the operation cannot have been properly performed. In an ear otherwise healthy, the operation is attended with so slight a degree of pain, that, when it has been performed in one ear, the patient expresses no unwillingness to submit in the other. The sensation it occasions is momentary; and no subsequent inconvenience of any kind arises: though if the ear have been previously irritated by stimulating applications, the operation will be painful, and should be deferred, till the inflammation has subsided.

As this operation affords relief only in cases of closure of the Eustachian tube, attention should be paid to the following criteria of this defect.

If a person, on blowing the nose violently, feel a swelling in the ear, from the membrana tympani being forced outward, the Eustachian tube is open. Though the tube be closed, if the beating of a watch placed between the teeth, or pressed against the side of the head, cannot be heard, the operation cannot relieve, as the power of the auditory nerve must have been destroyed. In a closed Eustachian tube there is no noise in the head, like that accompanying nervous deafness. Lastly, it is right to inquire, whether the deafness were immediately preceded by any complaint in the throat.

The

The causes of deafness are numerous, and many cases may be relieved by surgery. Nervous deafness.

The causes of deafness are extremely numerous ; and many of those which affect only the meatus auditorius, the membrana tympani, the cavity of the tympanum, and the Eustachian tube, admit of relief from surgery.

But there is one species, in which it would be absurd to expect this from any operation upon the membrana tympani. This occurs more frequently than any other, happening generally in old persons ; but sometimes, in the delicate and irritable, in the earlier stages of life. Anxiety and distress of mind have been known to produce it. Its approach is generally gradual ; the person hears better at one time than at another ; a cloudy day, a warm room, agitated spirits, or the operation of fear, will produce a considerable diminution in the powers of the organ. In the open air the hearing is better than in a confined situation ; in a noisy, than in a quiet society ; in a coach when it is in motion, than when it is still. A pulsation is often felt in the ear ; a noise, resembling sometimes the roaring of the sea, and at others the ringing of distant bells, is heard. This deafness generally begins with a diminished secretion of the wax of the ear, which the patient attributes to some unusual exposure of the head to cold ; and this continues as long as the disease remains. It may be cured in the commencement by the application of such stimulants, as are capable of exciting a discharge from the ceruminous glands ; for which purpose they should be introduced into the meatus. But if these be used so as to irritate, without exciting a discharge, they are rather prejudicial than otherwise. In cases of this kind the operation has afforded no farther relief than diminishing the noise in the head.

May be cured in the commencement by stimulants.

Deafness from the generation of a solid substance in the labyrinth.

Another cause of deafness is probably irremediable. This is the generation of a solid substance in the labyrinth of the ear, instead of the water with which it is naturally filled. Mr. Cline, dissecting the head of a young man born deaf, and consequently dumb, found all the parts of the organs of hearing perfectly formed, but the vestibule, cochlea, and semicircular canals, were filled with a substance of the consistence of cheese.

These instances of deafness Mr. Cooper thought it right to describe, because they are liable to be confounded with that which arises from a closed Eustachian tube.

EXPLANATION OF THE FIGURES. Plate VI.

Fig. 1 shews the external ear, the meatus auditorius, membrana tympani, and Eustachian tube.

A, the meatus.

B, the membrana tympani.

C, the cavity of the tympanum.

D, the Eustachian tube.

Fig. 2 shews the perforating instrument as it is introduced in the operation.

Fig. 3 the membrana tympani of Mr. G——, of which only that part which appears of a lighter colour remains.

Fig. 4 the membrane lacerated by a blow.

Fig. 5 the membrane lacerated in an attempt to extract a pin.

Fig. 6 shews the membrana tympani of a medical man in the city, having a fungus projecting through it. In this ear he is considerably deaf.

Fig. 7 the other membrane of the same gentleman.

Fig. 8 one of the membranes of Mr. P——, whose case was described in the former paper.

Fig. 9. A membrana tympani in its natural state, shewing the attachment of the manubrium to the malleus.

Fig. 10 the appearance of the membrane after having been punctured.

VI.

Note respecting the Absorption of Nitrous Gas, by Solutions of Green Sulphate and Muriate of Iron. By Mr. H. DAVY, Director of the Laboratory, and Lecturer on Chemistry to the Royal Institution.—(From the Author.)

WHEN nitrous gas is brought in contact with solution of green sulphate, or green muriate, of iron, it is rapidly absorbed; the colour of the fluid alters; and it becomes, when saturated with the gas, dark brown, and almost opaque.

Solution of green sulphate, or muriate of iron, absorbs nitrous gas;

Solution of sulphate, or muriate, of iron, impregnated with nitrous gas, apparently undergoes no change at low temperatures, when preserved from the contact of the atmosphere.

and undergoes no change at low temperatures.

But

unless exposed to the air, when it absorbs oxygen, becomes acid, deposits red oxide, and affords a little ammonia.

The impregnated solution, *in vacuo*, emits its nitrous gas without change.

The same solution, heated, emits nitrous gas, deposits a little red oxide, and contains ammonia.

Theory.

The nitrous gas is first absorbed by simple affinity; probably from the attraction of the iron for oxygen.

Exposure to air converts the nitrous gas into acid; a portion of which, and also of the water, are decomposed by the iron converted into red oxide.

Heat increases the tendency of nitrous gas to fly off, and also that of the iron to decompose it, and water.

Hence red oxide is formed by the oxygen of this gas and of the

But if it be exposed to it, it rapidly absorbs oxygen, loses its colour, and becomes acid to the taste. In this case, the green oxide of iron is converted into red oxide; and a small quantity of ammonia is formed.

When a solution of sulphate of iron, impregnated with nitrous gas, is placed under the receiver of an air pump, in proportion as the pressure of the atmosphere is removed from it, it gives out its nitrous gas. When the mercury in the gage stands at about $\frac{1}{2}$ of an inch, it becomes almost wholly freed from it, recovers its colour, and is found unaltered in its properties.

When solutions of green sulphate, or muriate, of iron, impregnated with nitrous gas, are exposed to the heat of a spirit lamp, nitrous gas is produced from them in a very pure state, the fluids lose their dark colour, and a small quantity of yellow oxide of iron is thrown down in them. If examined after this process, they are found to contain a little ammoniac, combined with some of the acid; and the oxide of iron dissolved is green oxide.

The theory of these appearances is obvious.

The absorption of nitrous gas, by solutions containing oxide of iron, at its minimum of oxidation, at common temperatures, appears to be owing to a simple combination between the gas and the fluid; owing probably in some measure to the modified affinity of the green oxide of iron for oxygen; for the red sulphate and muriate of iron have no affinity for nitrous gas.

The changes taking place in the impregnated solutions, when exposed to the atmosphere, evidently depend upon the conversion of the nitrous gas of the solution into nitrous acid, by the absorption of oxygen; and upon the subsequent decomposition of a portion of this nitrous acid, and of the water of the solution, by the green oxide of iron.

Heat seems to act upon impregnated solutions, by increasing the tendency of nitrous gas to assume the elastic form, and by increasing the affinity of the green oxide of iron for oxygen. In consequence, a portion of nitrous gas flies off, and another portion is decomposed; its oxygen combining with the green oxide, and its nitrogen with the hydrogen of a small quantity of water, which is acted upon by a similar attraction.

The absorption of nitrous gas by solution of common sulphate of iron was discovered by Dr. Priestley. It has been employed

employed by Mr. Humboldt for the purpose of ascertaining the quantity of nitrogen mingled with nitrous gas; and that experimentalist, in conjunction with Mr. Vauquelin, has attempted to prove that the process of absorption is owing to an immediate decomposition of the nitrous gas by the water of the solution; in consequence of which nitrate of ammonia is formed.

This conclusion is published in the 28th volume of the *Annales de Chimie*. In the 39th volume of the same work, 30 Messidor, An 9. p. 1. Mr. Berthollet states, that nitrous acid is probably formed during the absorption of nitrous gas by solution of sulphate of iron; in consequence of the evolution of a portion of its nitrogen, and in consequence of the more intimate union of its remaining constituent principles. Yet, in the same paper, he details an experiment on the application of heat to an impregnated solution, which was attended with results similar to those described above; and in consequence of which he is induced to enquire, "If the sulphate of iron is capable of exerting two different actions in nitrous gas; one tending simply to absorb it, and the other to decompose it?"

p. 11.

The general account that has been given in this Note of the true nature of the process, is extracted from "Researches, Chemical and Philosophical, concerning Nitrous Oxide, published in June, 1800." The experiments upon the absorption were made under Mercury; and, in consequence, the agency of the atmosphere, which appears to have misled the celebrated foreign chemists just quoted in their conclusions, was avoided.

VII.

On certain Metallic Sulphurets. By PROUST, Professor of Chemistry at Madrid*.

I LONG supposed, that the iron in pyrites was oxidized at the minimum, and, supporting my opinion by some analogies, I published it in the year 1795; but when my attention was recalled to the subject, by reading the Memoir which Vau-

* Journal de Physique. Thermidor. 9.

quelin has published on the sulphurets, I thought I ought no longer to delay making known the facts which have induced me to adopt a different opinion. I made use of the fine cubical and dodecahedral pyrites, which are obtained in abundance from an indurated argill, found in the vicinity of San Pedro Marino, near the town of Soria.

**Distillation of
pyrites**

Four hundred grains of entire pyrites of the size of hemp-seed, distilled by a red heat, yielded,

Of Pure sulphur	-	75 grains
Residuum	-	322
Loss	-	3

Total 400

Four hundred grains of the same of a little larger size, yielded,

Sulphur	-	81 grains
Residuum	-	314
Loss	-	5

400

A moderately red heat is sufficient for extracting all the sulphur which they are able to yield. In the first moments, a small quantity of aqueous vapour passes over; afterwards, two gases, which may easily be recognised by their smell, namely, the sulphureous and hydrogen-sulphureous, which mutually decompose each other in the water of the receiver, and render it milky. Both are owing to the decomposition of the humidity; the sulphur passes over afterwards, and the gas is no longer perceived.

**Converts them
into sulphuret.**

The abstraction of about 20 parts in 100 of the sulphur from the pyrites, destroys in a great measure their metallic lustre and solidity; and though they preserve their form, they are more voluminous, melted on all sides, and may be crushed between the fingers; they have only the dull colour of an artificial sulphuret.

**They contain no
oxygen.**

Hence it is evident, on comparing the products, that if oxygen constituted part of the pyrites, we must seek it in the residuum of their distillation; but the following facts annihilate the hope of finding it there.

Proof.

When we treat the different oxides of iron with sulphur or cinnabar, in a red heat, the results are, sulphureous gas, and common sulphuret of iron. If therefore the pyrites contained oxygen,

oxygen, it is evident that the latter could not escape the attraction of the combustible substance in a high temperature.

When treated with carbon, pyrites produces only sulphurated hydrogen; but if first deprived of 12 or 20 hundredths of sulphur, it presents only sulphuret, or iron saturated with sulphur, in the proportion of 60 to 100. It is soluble in the sulphuric, muriatic, and dilute nitric acids, affording sulphurated hydrogen in abundance.

Pyrites, therefore, according to these results, is nothing more than a sulphuret formed by nature in the humid way, and surcharged with an excess of sulphur, as if to ensure the duration of her work.

The pyrites will undoubtedly be found to differ greatly from each other with respect to this excess of sulphur; for, according to Henkel, there are some which yield 25, 28, even 32 per cent. of sulphur. It is therefore to this surplus that the product of this distillation belongs, and not to the sulphur, which has been combined with iron by the invariable law of the proportions; for the metal, when saturated with $\frac{60}{100}$ of this combustible, yields none on being exposed to that temperature which deprives the pyrites of its excess.

The pyrites, besides their different known uses, are of great use in a laboratory. We may fill a crucible with them, cover the powder with about a third part of filings, sprinkle over these a little charcoal powder, and afterwards bring the whole to a red heat, without carrying it so far as to reduce them to the state of fusion. In this manner we obtain a homogeneous mass, very convenient for procuring hydrogen in abundance.

By what I have just said, I do not however mean to imply that all the pyrites resemble each other. Those, for example, which possess the property of becoming vitriolized, approach perhaps the nearest to the sulphuret of iron without excess; for it is certain that the pyrites most charged with sulphur are at the same time those which resist the tendency of the elements to destroy them the longest.

The pyrites of Soria contains a little chalk, sand, and argill, but not the slightest trace of copper: its solution in the nitric acid is reduced to the minimum of oxidation by means of sulphurated hydrogen; and the yellow powder which separates from it is nothing but sulphur.

The

Natural production of red ochre. The pyrites undergo a slow decomposition, by which oxygen is substituted in the place of the sulphur, without their losing any thing of their dimensions; they are then nothing more than red oxide. When the waters transport, divide, and attenuate the oxidized pyrites, they become changed into red ochre: of this kind is that which in Spain is termed *Almagné*, from the name of the village where it is found. It is employed for painting houses, marking sheep, colouring the Seville snuff, polishing glass, &c. This earth, when distilled, yields sulphureous acid in abundance: it announces its origin by the remnant of sulphur which it contains.

Yellow ochre. As to the beautiful yellow ochres, which acquire a red colour by merely losing their humidity, they proceed from the spontaneous decomposition of the spathose ores: we therefore find them to contain lime and manganese. I have observed this conversion of the spathose ore into the state of yellow ochre in a very distinct manner in certain veins; that is to say, the oxides of iron and manganese, when both at the minimum, and saturated with carbonic acid, raise themselves to the maximum, by abandoning the acid, which can no longer remain united with them in this state.

Umber.

Analysis of umber shews it to be vegetable. If this earth be a vegetable residue, as cannot be doubted, its origin is well confirmed by its analysis, for it is a compound of oxide of iron and manganese, both at the maximum, of argill, of sand, &c. As to the argill, it abounds in the ashes of certain kinds of wood; I have found it in large quantity in that of the olive and cereb trees; in that of the holm (*encina* in Spanish) I have found the calcareous phosphate, which I neglected to announce at the proper time. In addition to my second memoir on Prussian blue, I have to add, that ox's blood certainly contains manganese.

Sulphuret of Mercury.

Mercury in cinnabar is not oxidized. Artificial cinnabar constantly yields 85 per cent. of mercury on the one hand; on the other it relinquishes 14 and $14\frac{1}{2}$ of sulphur to the antimony which is used for effecting its decomposition. The mercury is not therefore originated in cinnabar.

In

In my youth I made two experiments, which might have cost me very dear. I wished to convert mercury into cinnabar in the dry way, as I had done by employing the oxides in the humid way. Four ounces of corrosive muriate, and four ounces of red oxide, triturated with sulphur, were exposed to the fire, each mixture in a separate retort. In less than half an hour, two explosions took place, which were heard throughout the extensive works of La Salpêtrière. My brother, who conducted the first experiment, being suddenly overwhelmed with a thick smoke of sublimate, had nearly lost his life, and his lungs were long afterwards disordered. The result of my operation was, that the dome of the furnace was thrown into the air, whilst the door of the ash-pit was shattered against a wall, and narrowly escaped striking me in the stomach.

The powder which arose from the detonation of the oxide mixed with sulphur, according to the method of Bayen, is cinnabar of a violet-red colour.

The æthiops, prepared with the aid of fire, reddens spontaneously in course of time, in the oxidizing vessels.

Mercury poured into a retort with melted sulphur always occasions an inflammation; but if both have been previously heated, the union takes place, and the æthiops affords cinnabar without flame. As to that which accompanies the process used by the Dutch, it deserves to be better examined than has hitherto been done.

Hydro-sulphurated water, poured gradually into a solution of sublimate, decomposes it entirely. The precipitate is mild muriate, and muriatic acid is disengaged. This result proves, that there is not only more oxygen in the oxide of sublimate, but also, as Berthollet has remarked, more marine acid. If, on the contrary, we pour the solution of sublimate into the hydro-sulphurated water, the whole becomes precipitated in the form of æthiops, and the muriatic acid remains alone.

The name of *sweet* muriate brings to my recollection a fact recorded by Lemery in the Memoirs of the Academy. He knew an alchemist, who eat the mild muriate like bread; he has seen him swallow four ounces at a time, and he assured him that he took from time to time a like dose, to purify his blood, as he said. This anecdote, the authenticity of which cannot be suspected, induces me to ask what we are to think of the 18 and 24 grains of *mercurius dulcis* which physicians

Dangerous explosions, by attempts to form cinnabar with oxidized mercury, by heat.

On the inflammation of the common process.

Corrosive sublimate contains more oxygen and more acid than the mild muriate.

Remarkable fact.

Anecdote, to shew the inefficacy of mild muriate as a medicine. It was not the muriate. W. N.

are in the daily habit of prescribing? I greatly fear that it may be much the same with regard to its effects as with those of sedative salt, and so many other medicines, which have nothing to recommend them, but the honour which is done to them, of affording them a place, from age to age, in our dispensatories and catalogues of the *materia medica*.

Mild muriate
treated with sul-
phur;

I find also in my notes of the same period, that the mild muriate, treated with sulphur in a retort, affords cinnabar and corrosive muriate; the mild muriate is therefore divided into two portions; one of which is converted into cinnabar, and the other charges itself with the oxygen and acid of the first.

also turpeth.

That turpeth mineral, treated in the same manner, is changed into cinnabar, and quits the oxygen, which goes to form sulphureous acid.

The concentrated sulphuric acid decomposes cinnabar, like the pyrites and other sulphurets, yielding to the metal the oxygen which they require; hence the sulphureous acid, &c.

Cinnabar in the humid way.

Mercury prefers
sulphur to oxy-
gen.

Mercury having much more affinity for sulphur than for oxygen, never fails to shew its preference for this combustible. It is not subject to the law which enables zinc, tin, antimony, &c. to combine with sulphur, without abandoning the oxygen.

Liquid sulphu-
rets decomposed
by crude mer-
cury;

F. Hoffman was, I think, the first who thought of decomposing the ammoniacal sulphuret by means of mercury. Baumé and Wiegleb have since occupied themselves with the subject, and in a more extensive manner.

which takes the
sulphur, and be-
comes cinnabar.

Mercury, poured into flasks containing sulphuret of potash, or of ammonia, decomposes them entirely, and reduces them to potash and ammonia: in the space of a few days it passes from the black to the red colour. If mercury required oxygen in order to assume the colour of cinnabar, whence should it take it? This process is certainly of a very difoxidating nature.

Mercurial salts
and oxides also.

The nitrates, muriates, sulphates, and mercurial oxides of every kind, all yield æthiops in the first moment of their mixture with the sulphurets: they all become violently heated. In order to accelerate their coloration, it will be sufficient to place the flask with the mixture upon burning coals; in an instant they begin to grow red at the points where the heat exerts its action. Is it sulphurated hydrogen which they then lose? For my part, I cannot tell.

ALL

All these cinnabars differ with respect to their shades of colour and fineness; some are of a tinge approaching to purple, violet, &c. That of sublimate combines with a great degree of tenuity, a vivid scarlet brilliancy. It is much superior to the richest vermilions, and it is to be wished that painters might become acquainted with it.

These cinnabars differ. A very beautiful pigment.

Others are pulverulent, of a dark or dull colour; such are those which the sulphuret of potash yields; they have neither the lustre nor the tenuity of the others, for they are crystalline. For the rest, all these cinnabars present no extraordinary appearances in sublimation; by this process they lose the brilliancy which they owed to their state of division.

Tin, which has so great an affinity with oxygen, takes however nothing else than sulphur from cinnabar; if the latter contained also oxygen, it would take it from it with the same facility as it does the sulphur; and the presence of the one would certainly be no impediment to the tin becoming charged with the other: for if we heat a mixture of cinnabar and oxide of tin at the maximum, we obtain mercury, sulphurated oxide of tin, and sulphureous acid. This last is here produced, as in the case where sulphur is treated with the oxide of tin, by a reduction which brings this oxide from the maximum to a minimum of oxidation, of which I do not yet know the numerical value.

Tin takes sulphur from cinnabar; but no oxygen: that principle not being present.

Sulphuret of Arsenic.

This is also one of those which have long deceived me by the analogy which its transparency and colour seems to indicate.

The sulphurets of arsenic contain no oxygen.

The arsenical acid and white oxide, treated with sulphur, lose their oxygen, yield sulphureous gas, and are reduced into a sulphuret, which is transparent, vitreous, of a yellowish red colour, and capable of being raised in distillation.

Pure arsenic, treated in the same manner, yields the same kind of sulphuret, of perfect volatility, colour, and transparency.

In whatever ways we may expose these sulphurets to the action of pulverised charcoal, iron, tin, &c. they remain unalterable. Minute portions of sulphureous gas, and sulphurated and arsenicated hydrogen, appear at first in consequence of the moisture, but nothing more.

The native, scaly, flexible orpiment melts quietly, and forms a vitreous, red, transparent mass, resembling realgar, and unalterable by the action of charcoal. There may exist natural sulphurated oxides of arsenic, but certainly these are not such as are formed by an elevated temperature. The native realgar of Ronda in Andalusia, is also a metallic sulphuret without oxygen.

Sulphuret of Copper.

The sulphuret of copper exhibits a second instance in the mineral kingdom of sulphurets with a surcharge of sulphur.

Characters of
sulphuret of cop-
per.

As the lights of analysis have hitherto been wanting, in order to point out to mineralogists the real characters of this sulphuret, they have always described it in an imperfect manner, or confounded it with other mineral productions. In its purest state, its colour is always a deep blue, violet, or mixed with the copper tinge of indigo, if rubbed with a smooth substance. Its colour is liable to be disguised by a mixture of carbonate of copper, red oxide of iron, &c. But when we separate these by solution, the sulphuret is restored to its real colour.

Variations from
impurity.

More commonly the sulphuret of copper is disguised by its intimate combination with the other metallic sulphurets.

Combined, for example, with the sulphuret of iron, it affords the copper-coloured pyrites, in which the existence of the blue sulphuret may easily be demonstrated.

If with these two sulphurets, considered as the base, are combined those of antimony, lead, arsenic, mercury, silver, zinc, &c. each separately, there results a series of ores of copper, rendered grey by as many different metals, but which do not on that account, as mineralogists believe, necessarily contain silver.

Compound ores
of copper.

If with the two first sulphurets we combine in idea the 2d, 3d, 4th, and 5th of these which we have just mentioned, we have in these combinations as many complicated ores of copper. Amongst those which are brought from America, there are others likewise compounded with native silver, the carbonates of iron, manganese, sulphate of barytes, &c. Such are the different species of mineralizations which mineralogists have indiscriminately comprehended under the two classes of white and grey ores of copper.

The

The grey ore of copper of La Cren, in the kingdom of Valentia, is of this kind: it is composed of four sulphurets, that of copper, of iron, of antimony, and of mercury. Deposited in masses in the calcareous breccias formed by the remains of mountains which no longer exist, it undergoes a decomposition at its surface, which converts three of the sulphurets into oxides, whilst that of mercury remains entire, and imparts to the decomposed ore a brilliant vermilion hue. At the centre there is always a nucleus composed of these four sulphurets in their entire state. The analysis of this mineral, given by Fernandez, is in the *Annales*: but I return to the sulphuret of copper, from which I did not intend to digress.

Those which I have had opportunity to examine contain from 14 to 15 per cent. of sulphur, which is easily extracted from them by a moderate heat, and what remains in the retort is always the blue sulphuret, saturated according to an uniform proportion. This proportion is 19 per cent. like that of the artificial sulphuret. If a native sulphuret of copper leaves 86 of residuum, we may precipitate its nitric solution by sulphurated hydrogen; and the precipitate separated from the excess of sulphur re-produces 86 parts of sulphuret of copper. Hence we see, that though the native sulphuret is subject to an excess of sulphur, it does not on that account differ from the artificial sulphuret, when it has been deprived of this excess.

Copper pyrites, like that of iron, contains an excess beyond the state of sulphuret.

By the action of a red heat, the oxides of copper, mixed with sulphur, yield only blue sulphuret. This metal does not afford a sulphurated oxide, either in nature or in the products of art. Its formation is always attended with a considerable extrication of heat. It is astonishing, if it be duly considered, that it could ever for one moment have been mistaken for a combustion.

Copper sulphurets contain no oxygen.

The blue sulphuret dissolves in copper, and forms the black coppers, independently of the iron which they may contain. Acids of 10 or 12 degrees separate it from these coppers without decomposing it.

Black copper.

On the Fuming Muriate of Tin.

Since the year 1777, I have been in the habit of preparing this muriate with corrosive sublimate and pulverised tin; and though I never thought of publishing this process, it was because I imagined it to be in common use in every laboratory.

Fuming muriate of tin prepared without amalgamation.

But

But as, in the latest treatises of chemistry, the ancient process of amalgamation is still prescribed, I think it may be useful at present to recommend the use of the mere pulverised tin.

These are the details which I find in my notes of that period; they shew that the following proportion is the best:

Instructions. Twenty-four ounces of sublimate and eight ounces of tin yield nine ounces of fuming liquid.

Conceiving that some advantage might be obtained by augmenting the proportion of sublimate, on account of the great excess of pure and semi-oxidised tin which is found in the residuum, I made the following trial:

Experiment. Thirty-two ounces of sublimate and eight ounces of tin yielded ten ounces of the liquid; hence it appears, that the expenditure of eight ounces of sublimate is not compensated by more than one ounce more of liquid.

The acid is not oxidised in the fuming muriatic. I shall shew, upon another occasion, that in the fuming muriate of tin, as well as in the corrosive muriate of mercury, the bases only are oxidated at their maximum, whilst the muriatic acid remains in its ordinary simple state.

VIII.

A Memoir on the Method of Bleaching the Paste of Paper.

*By CITIZEN LOYSEL *.*

Berthollet's bleaching process.

THE advantages of the method of Citizen Berthollet for bleaching thread and piece goods, by means of the oxygenated muriatic acid are universally known. Citizen Chaptal has usefully applied this invention, to restore the colour of prints and printed books; and he has also simplified one of its most important parts in his new processes respecting the art of applying the lixivium.

Advantages of applying it to paper.

The adoption of Citizen Berthollet's method to the art of paper-making, is capable of carrying this branch of industry to a still greater degree of perfection, particularly in France. We are abundantly supplied with the raw materials proper for manufacturing paper; but according to the present processes of our paper mills, there is only a very small portion of the

* Translated from the *Annales de Chimie* XXXIX. 137.

rag which can be used to manufacture fine white paper. All the residue is condemned to the fabrication of papers of inferior quality.

The process of bleaching the paste of the paper maker, even when produced from the most common rags, will communicate to it the quality of the best sort. By these means our paper manufactories may supply our wants in fine white paper, and even obtain the preference in foreign markets. The result of this operation would be, that a greater number of workmen would find employment, and the advantages of this increase of industry would be of still greater national value, than even the foreign export which might be expected.

The success of bleaching the paste of paper by the method of Citizen Berthollet is no longer problematical. The application which has been made to the paper used in making assignats, has placed this question beyond all doubt as to its solution.

It was at the commencement of the year 2, that the committee of assignats and monies of the National Convention, which I was a member, resolved to employ this method, together with that of stercotype which had been adopted, to oppose new obstacles to the practice of forgery.

We particularly consulted Citizens Berthollet, Fourcroy, and Guyton on this enterprize. Their approbation of the project, and the information they afforded us, soon gave us the power of realising it. We were also assisted with the knowledge of Citizens Welter, Athenas, Alban, Carny, Marchais and Ribaucour, who with great zeal communicated their processes, and permitted us to inspect their several manufactories.

I shall not here relate all the previous experiments we made before we established our works on a large scale, but shall content myself to point out concisely the manner of our commencement, the agents we employed, and the means which to us appeared the most simple to obtain our desired purpose. I shall afterwards add a few observations respecting the means of still greater perfection, which the advancement of the science since that time has rendered the same operations capable of.

Our first processes were executed precisely according to the method of Citizen Berthollet. This rag was subjected in succession to the first trial.

Difficulty of
using the bleach-
ing liquor with-
out alkali.

succession to different lees, to baths of the bleaching liquor, and sulphuric acid pointed out in his memoir. Berthollet had shewn, and we were also convinced by our own experience, that the gas is less confined in the simple fluid, prepared without addition of fixed alkali, than it is in that which contains potash or soda; and that it is consequently more disposed to separate and enter into new combinations. We therefore at first made use of this simple liquor; but the workmen soon exhibited a strong repugnance to its use on account of the fumes it emits, which are extremely inconvenient, even when chalk is diffused in the liquor. This inconvenience forced us to abandon it, though with regret. This sacrifice was so much the more considerable, as it occasioned a loss of time, and considerable increase of expence. We decided that we would receive the gas in a solution of potash; but as the doses in which this alkali may be used have limits of great extent, we endeavoured to keep as near as possible to that preparation which is sufficient to prevent the spontaneous disengagement of the gas, and by that means cause the liquor to lose the odour we were desirous of avoiding. This dose was 5 kilograms of potash to 100 litres of water, (11 pounds avoirdupoise, to 21½ ale gallons.)

The rags became
very white, but
when opened by
the mill were
less so.

The rags bleached in this manner became of the most brilliant white. Nevertheless, a part of this perfection disappeared, when the rag was converted into paste, and that paste into paper. It was easy to discover the cause; namely, that the interior parts of the thread in the rag were less exposed to the action of the liquor than those at the surface. This motive determined us to abandon the bleaching of the rags, and to operate upon the paste itself.

The paste was
therefore
bleached

We were here opposed by new obstacles. When the rag is converted into a paste proper to be worked, its coherence is such that it settles, and no longer permits the lees and baths of the bleaching liquor to penetrate through all its parts, in consequence of which property the paper was found to have veins and different shades of colour. We remedied this inconvenience, by taking the matter in a mean state between the rag and the paste proper to be converted into sheets of paper. We succeeded in this respect by destroying the texture of the rag under the first cylinder so as to separate its fibres, an operation which usually lasted two hours for a pile of 50 kilograms.

In its half pre-
pared state.

Thus

Thus it was, that by successively avoiding the extremes of too much and too little mechanical connection, we advanced towards our object.

The apparatus which Citizen Welter imagined,* and of which Citizen Berthollet has given a description in the first volume of the Journal of Arts and Manufactures, is applicable to all the methods which can be employed to procure the different kinds of bleaching liquor, whether the water of the receiver contain fixed alkali or not; whether the muriatic acid be used on the oxide of manganese, or the gas be obtained by sulphuric acid, upon the mixture of oxide of manganese and muriate of soda. This apparatus is particularly preferable to all others in the case where the water of the receiver contains no alkali, because the absorption of the gas is favoured by its being brought into contact with the water at a great number of surfaces. But as we had determined to use a solution of potash, we were able to make some modifications of this apparatus.

1. The three inner partitions of the receiving vessel were reduced to one only. Ours was formed of a tub containing another tub inverted, and both were covered with sheet lead.

2. The size of the tubes of communication was so considerable, that we had nothing to fear from absorption during the course of the distillation.

3. Lastly, the practical skill of our workmen in managing the fire, and the advantage of having only one opening to the lute enabled us to suppress the intermediate vessel.

Description of the apparatus used in preparing the oxygenated muriatic acid in bleaching the paste of paper for assignats.

Fig. 1, Pl. VII. Plan of the apparatus.

Plan.

1, 1, 1, &c. Eight furnaces, having a chimney of sheet iron common to each pair of furnaces.

2, 2, 2, &c. Eight vessels of cast iron, containing sand.

3, 3, 3, &c. Eight matrasles, balloons, or bottles of stone ware, compact and well baked, intended to contain the materials which afford the gas. Each matras must be filled only to one-third of its capacity at most. Bodies of glass of little thickness may also be used for this purpose.

4, 4, 4, &c. Tubes of glass to conduct the gas into the receiver. Or these tubes may be made of lead.

5. The

Plas.

5. The receiver. It is composed 1. of an external vessel, covered with plates of lead well foldered together, and provided near its bottom with a cock 6, to draw off the liquor when prepared. 2. Another vessel, 7, likewise covered with plates of lead within and without. This second tub is inverted in the first to contain the gas in proportion as it is disengaged, and to keep in contact with the water of the receiver, that portion of gas which had not time to be dissolved, in passing through that fluid.

There is a hole, 8, in the upper part of this second vessel. It serves to suffer the common air to escape when water is first poured into this receiver, and it is afterwards closed with a stopper of lead or cork, covered with paper, soaked in starch, and fastened to the cork by a piece of cloth or bladder, before the operation began.

Vertical section. Fig. 2. Vertical section of the apparatus.

Fig. 3. Elevation of the apparatus.

The disposition of the furnaces about the receiver, and the circular form of the receiving vessels, was rendered necessary by the local circumstances of the laboratory in which our operations were carried on. In other circumstances square vessels might be employed, and all the furnaces might be ranged in a right line under a common chimney.

One thousand litres of water are placed in the receiver, holding in solution fifty kilograms of white purified and calcined potash.

When the disengagement of gas is effected by the muriatic acid, the materials are used in the following doses :

Oxide of manganese	- - - - -	24 kilograms
Muriatic acid at 20 degrees of density according to the barometer of Baumé,	} - - - - -	68 — 92

which makes for each of the eight distilling vessels $11\frac{1}{2}$ kilograms of materials.

Charging and
distillation.

The operation is begun by charging the receiver with 1000 litres of alkaline water, after which the aperture 8 is closed with its stopper well luted. Each matras is then placed in its sand-bed; and pulverized manganese is introduced. The muriatic acid is poured upon the manganese, and the stoppers into which the tubes of communication pass, are duly placed. The juncture is luted with paper soaked in starch.

March. And the lute is left to dry from six to twelve hours, after which the fire is lighted in the furnaces.

The process of distillation lasts from ten to twelve hours. When it is finished the tubes are unluted, the fire extinguished, and the matrasles suffered to cool in their sand beds, till the temperature of these beds has descended to 60 or 70 degrees, (centigrade) at which period, water of the same heat is poured into the matrasles. The water dilutes the residue of the distillation, which mixture is to be poured out, and the vessels suffered to cool in baskets containing straw. If the precaution of introducing hot water in this manner upon the residue were not taken, it would become so solid when the operation is performed with sulphuric acid, in the manner we are about to describe, that it could not be extracted without much trouble and danger of breaking the vessels.

If the disengagement of the gas be made by sulphuric acid, Process with sulphuric acid. the following doses are used :

Oxide of manganese	- - - -	25 kilograms
Muriate of soda	- - - -	70
Sulphuric acid at 50 degrees of density		25
Total		120

The acid is to be diluted with an equal bulk of water, or 16 litres, which will reduce its density to 31 degrees.

The eighth part of this for each matrasle amounts to 14½ kilograms.

The oxide of manganese and muriate of soda being pulverized are mixed together. The matrasle is to be charged and the operation conducted as before described. This method is the most economical, because the sulphuric acid is cheaper than the muriatic, and also because it is practicable to obtain from the residue of the distillation, the soda of the muriate which is converted into sulphate of soda; that salt being decomposable by well known processes.

In order to measure the force of these liquors, or their bleaching power, we made use of the solution of indigo prescribed by Citizen Desroisilles, which is prepared in the following manner: To ascertain the strength of the bleaching liquor

In a glass matrasle put of concentrated sulphuric acid at 66 degrees of density, seven parts by weight, and of pulverized indigo one part; agitate the mixture, and plunge the ball of the By indigo.

the mattrafs to half its depth in water moderately heated, agitating it from time to time. The solution will be effected in two hours, after which it is to be diluted in 992 parts of water. This is the proof liquor. The stronger the bleaching liquor, the greater number of parts of the solution of indigo it will render colourless, and we may by this proof determine the doses of each kind of bleaching liquor to be employed, together with water, so as to compose a bath proper for immersing the substance intended to be bleached.

One part by measure of the bleaching liquid prepared as before mentioned, will usually destroy the blue colour of nine parts of the proof solution of indigo; it was of the same strength as that of Javel, prepared by Citizen Alban.

Choice and Preparation of the Rags.

Selection of the materials for paper, quality of rags, &c.

The strength or tenacity of paper depends upon the staple or fibre of the material from which it is made. Rags of new cloth and cordage compose a paper more tough than old rags, and the first of these materials presents a great variety on account of the quality of the hemp or flax of which they are formed. Rags of fine new cloth, whether raw or bleached by the oxygenated muriatic acid, stand in the first rank, after which cordage and old rags may be classed.

Bank or note paper.

Paper intended for bills of exchange, or other commercial and legal instruments ought to be tough, in order that it may not be easily torn when thin, for this paper the materials of the first class must be intirely, or in large proportion employed. The price which the consumers are disposed to pay for this article, is sufficient to indemnify the manufacturer for his care and industry, as this kind of paper is sold in France for 5 or 6 francs the kilogram.

Common paper.

The other papers also require to be more or less tough, according to their thinness, and the use to which they are applied, but a clear white colour is sought in paper of every description. The first operation to which the rags are subjected is sorting, in order that each branch of the manufacture may have its appropriate material, after which they are cut with shears into pieces of about one decimeter, or three or four inches square.

I will suppose that the object of the manufacturer is to obtain paper of a beautiful white. If it is intended to be thin, so that,

that, for example, a ream of the size denominated *raisin* should weigh only four or five kilograms, that is to say, about one-third of the weight of common paper of the same form. The manufacturer makes choice either of new rags already of a fine white, or of unbleached rags.

In the case of the white rags, it is sufficient to pass them under the first cylinder, then to give them a bath of the bleaching liquor, and afterwards a bath of sulphuric acid, as we shall proceed to direct, after which they are passed under the finishing cylinder for seven or eight hours, and, lastly, conveyed to the working trough to be made into sheets of paper.

Rags, which have never been bleached, may be treated by either of the following processes, that is to say, the first, which preserves the utmost degree of toughness to the paper, but is likewise the most expensive, consists in decomposing the rag, and afterwards applying the method of Citizen Berthollet for bleaching piece goods; namely, subjecting it to three or four lixiviations, and afterwards alternately to lixiviations, baths of the bleaching liquor, and baths of sulphuric acid. The weight of the raw unbleached material is diminished from 50 to 45 per cent. in these operations.

This method was the first which we used for the assignat paper; but we soon perceived that we might omit most of the lixiviations and baths of the bleaching fluid, and still preserve as much toughness as the paper required. Nothing further was necessary for this purpose than to suffer the rag to undergo a degree of fermentation more or less advanced, by leaving it to rot. In this operation the colouring matter undergoes a slow combustion, and passes to a kind of saponaceous state, and is carried off by the water, by washing the rags in the vessel of the first cylinder.

One single lixiviation, two baths of the bleaching liquor, and one of sulphuric acid, are then sufficient to bleach completely the raw rags or cordage. This is the second method. We were not, at that time, acquainted with the economical process of Citizen Chaptal in the operations of lixiviation. This will, no doubt, be used; but the effect of rotting, carefully conducted, will always be found very advantageous.

Lastly,

Lastly, if the rags be neither perfectly white nor raw and unbleached, but in a medium state, that they are left to rot for a shorter time, for example, twelve or fourteen days, and are taken up when the heat of the fermentation raises the thermometer to 30 or 35 degrees, after which the process is to be conducted as before mentioned.

Composition of a bath of the bleaching liquor, for a pile of decomposed rags, weighing 50 kilograms.

Application and proportions of materials in the actual work.

For each heap of rags a certain number, for example, eight or nine, wooden tubs are disposed in a line, capable of containing in the whole 600 litres of water: 450 litres of pure water is poured in, and 90 litres of bleaching liquor are added in equal portions to each of the vessels, after which the 50 kilograms of decomposed rags are disposed in equal portions in each tub. The stuff is left for about twelve hours in this bath, agitating it from time to time, after which it is to be completely washed in clean water, and put into a bath of sulphuric acid, composed of water 200 litres, and acid at 50 degrees three kilograms, which bath will then have the strength of about four degrees of the eareometer of Baumé.

The immersion in the bath must continue for three quarters of an hour or an hour, after which the materials must be well washed in clear water, and carried to the mill to be manufactured.

If the action of the baths of bleaching liquor be not exhausted by the immersion of the decomposed rags (which may be ascertained by the solution of indigo), it may be applied to other materials of the same kind.

Improvements.

Such was the state in which we left this new art in the year 3. Since that time Citizen Welter, to whom chemistry and the arts are indebted for a number of ingenious processes, has simplified that of preparing the bleaching liquor. He has found, for example, that instead of the three vessels of the receiver, it is sufficient to employ two even for the simple liquor that contains no fixed alkali.

Rise of the liquor without alkali.

It was before seen that we were obliged to employ an alkaline solution in the receiver, to prevent that odour which the simple liquor emits when paper stuff is agitated in the baths. The use of alkali answered our purpose very well in this respect;

spect; but this expenditure, besides weakening the bleaching liquor, nearly doubled our expence. Though this difference in the price was of little consequence with regard to the object we then had in view, it is not so with regard to the common operation upon paper intended for sale. Every means of economy must then be used. Now Citizen Welter found that it is easy to obviate the inconvenience of the simple liquor in the operation. His method consists in no longer agitating the goods or material in an open bath, but to clothe it exactly by means of a cover; and he agitates it by means of cross pieces attached to a handle turned on the outside.

A rough estimate of the price of the simple bleaching liquor prepared by the sulphuric acid, this being the most economical process. Estimate of charges.

The receiver is supposed to contain 1000 litres of water.

25 kilograms of oxide of manganese cost at most	-	-	15fr.	0c.
70 kilograms of muriate of soda	-	-	7fr.	0c.
25 kilograms of sulphuric acid, at 50 deg.	-	-	37fr.	50c.
Three days work, principal men	-	-	9fr.	0c.
Three days —, assistant or labourer	-	-	4fr.	50c.
Fuel, about	-	-	3fr.	
Wear and tear	-	-	6fr.	

Our apparatus cost 622 franks, and the carriage and fixing increased our expence to 1000 franks, the interest of which, at 10 per cent. is 100 franks; and if the work be repeated so many times in the year, the interest per operation will be

1fr.

83fr.

Hence the litre of bleaching liquor will cost nearly 9 cents in round numbers. *

* As the price of all these several items in France must materially differ from the same in England, I have thought it unnecessary to reduce the numbers.

Estimate

METHOD OF BLEACHING PAPER.

Estimate of the increase of expence occasioned in the operation upon a pile of 50 kilograms of the paste of paper, supposing one bath of the bleaching liquor and one of sulphuric acid, which is most commonly the case.

Ninety litres of the bleaching liquor at nine cents.	9c.
	8fr. 10c.
Three kilograms of sulphuric acid, at	
1fr. 50c.	4fr. 50c.
Workmanship	50c.
	<hr/>
Total	13fr. 10c.

Which gives for each kilogram of paper an expence of 0,262 franks, or about 27 cents. Now the common paper in the market usually sells for about 1 fr. 30c. or 1fr. 40c. the kilogram, and with the simple augmentation of 27 cents for the operations of bleaching, it obtains the preference beyond that which is sold for three, four, or even five franks, which can only be obtained in limited quantity, on account of the selection of rags. The foregoing methods must therefore produce a great diminution in the price of fine paper. They are more particularly advantageous when applied to the manufacture of thin paper, because the expences of bleaching are always proportioned to the weight of the material, and consequently are least upon thin paper.

Distilling apparatus of Athenas.

I shall conclude this memoir with a description of an apparatus, invented by Citizen Athenas, for preparing the oxygenated muriatic acid. He had the complaisance to lend it to us. This apparatus is remarkable for its simplicity, as it requires no vessels of earth or glass, nor intermediate vessels, nor tubes of communication.

Fig. 4, Plate VII. represents the horizontal section, and Fig. 5 the vertical section. The description of this last will be sufficient to explain the construction.

9 represents the ash-hole.

10 the fire-place, having its chimney turned on one side.

11. An iron boiler, containing water kept at the heat of ebullition, serving as a bath for the cucurbit, and effecting the disengagement of the gas.

12. The distilling vessel of lead, in which the subject matters of the operation are put.

13. A

13. A vessel lined with lead, intersected by the vessel 12, with which it is soldered.

14. A cock to draw off the fluid when prepared.

15. A receiver of lead inverted in the tub. A hole, 16, may be made in this receiver to let out the atmospheric air when it is plunged in the tub, which hole must be closed before the disengagement of the gas begins. The inverted receiver bears upon three projections of wood, which keep it at a little distance above the bottom of the tub.

When the strength of the liquor obtained by this apparatus was compared with ours, it was found that with equal proportions of materials and of water its discolouring action upon indigo was no more than about the half of ours. Consequently there is a real loss in the consumption of these articles. This

This last apparatus was less effective than the former.

must be attributed to the low degree of heat, which does not expel the whole of the gas. I have nevertheless judged that it would be of use to describe this ingenious mechanism, the simplicity of which will, no doubt, give satisfaction to artists.

It is obviously capable of improvement, either by placing on one side a separate furnace, with a sand and ordinary distilling vessel, whence the gas may be conducted beneath the receiver by a leaden tube; or, according to the actual disposition of the apparatus, a liquid or dry bath may be substituted to that of water, so as to communicate a higher degree of heat, provided the temperature be less than will melt lead, a condition which admits the desired extrication of the gas.

But it may be improved by raising the heat, and is very simple.

IX.

*On the Sound produced by a Current of Hydrogen Gas passing through a Tube. * With a Letter from Dr. HIGGINS, respecting the Time of its Discovery.*

INTO a glass bottle is put sulphuric acid and iron filings, through the cork of which a glass tube is passed; the upper extremity is capillary: then by setting fire to the hydrogen gas which escapes by this extremity, a continued current or jet

The flame of hydrogen passing through a capillary aperture, under a tube,

* Bulletin des Sciences, No. 56, An. 10.

ON THE SOUND PRODUCED BY HYDROGEN GAS.

of flame is produced, which is let pass into a tube of either glass, metal, earth, or any other substance; when the following phenomena are observed.

Is clear
142.

If the tube be not too large, the flame becomes smaller as it is depressed, and when it is much reduced, the tube emits very clear sounds.

But if the tube be too narrow, the flame will be extinguished, and in proportion as the tube is larger, the sound diminishes; so that there is a certain limit at which it totally ceases, which also happens when the tube is too long.

The sounds may be varied at pleasure, by either using tubes of different figures and dimensions, or made of various substances.

These experiments have been made in Italy. Citizen Brugnattelli had described them in his *Annals of Chemistry*; and he has repeated them with Citizen Volta in the cabinet of the Polytechnic School, in the presence of several persons.

LETTER from Dr. HIGGINS.

To Mr. NICHOLSON.

DEAR SIR,

IT was in the year 1777, that I first exhibited the experiment you mention, and produced several sweet tones, according to the width, length, and thickness of the glass jar or sealed tube, held by the bottom, inverted to some depth over the flame of hydrogen gas. This experiment was afterwards shewn by some of my pupils, to amuse their friends, and particularly by my cousin, Mr. Higgins, who now teaches in Dublin; who exhibited it at Sir Joseph Banks's, and, if I am not misinformed, at Oxford, where he served the professor as operator.*

of the
cry.

These tones first occurred unexpectedly, whilst I was making this easy and popular experiment, to shew the water formed during the slow combustion of the slender stream of hydrogen gas in the air of the jar. And as I always had a

* I remember the experiment at Mr. Kirwan's, in 1784, which led me to request the information here given.

great

great variety of such jars on the table, many different tones were produced on the very first day, by applying jars of different capacity and thickness.

I am, SIR,

Your's respectfully,

B. HIGGINS.

Percy Hotel, Jan. 12, 1802.

The cause of the curious phenomenon here described seems Speculation on its cause. difficult to be clearly ascertained. As it is universally admitted that sound is caused by undulations, that is to say, alternate condensations and rarefactions of the air, or some more subtle fluid, we are naturally led to an intension and remission of the flame of the hydrogen. It is probable that all flame undulates. The flame of a candle is said to dance Cause why the flame of a candle dances. when this effect is most remarkably evident. I suppose the supply of oily vapor enlarges the flame, until its surface becomes so extensive, as to absorb oxygen in greater quantity than the fuel can be supplied to keep up a combustion of that size: it therefore becomes less, until by the diminution of surface, the due proportion of oily vapor, compared with the oxygen, again exceeds. And in this way we may form a notion of perpetual undulations of size. If these changes were rapid and strong enough, the air would be struck by them into sonorous undulations. In the same manner perhaps A chirping noise produced by flame. we may account for the loud chirping noise I have observed to be produced by the inflamed drops of tallow which fall from a candle held nearly upside down at five or six feet from the ground.

W. N.

X.

Description of the Graphometer, or Instrument of CIT. CARANGEAU, for measuring the Angles of Crystals.—From the Minerologie of CIT. HAUY.*

Structure of the graphometer. Two concentric, equal quadrantal arcs joined by an hinge; to which are applied compass legs or radii, capable of being shortened to apply to small crystals, &c.

IN order to determine the mutual inclination of the faces of a crystal, or its prominent angles, an instrument is employed, for the invention of which we are indebted to Citizen Carangeau. This instrument, which considerably resembles the graphometer, consists of a semicircle *MTN*, (Pl. VIII. fig. 1.) of brass or silver, divided into degrees, to which two indexes, *AB* and *FG*, are applied, the one of which, *FG*, is hollowed out from *u* to *R*, from face to face, except at the place *K*, where a small portion is left for the purpose of giving greater strength to the instrument. This index is attached at *R* and *c* to a brass piece or rule situated behind, and forming the same piece with the limb. The connection of the index with this rule is effected by means of two small milled-head-screws, inserted in the groove. The other index, *AB*, is hollowed out in the same manner from *x* to *c*, where it is attached above the former, by means of the screw at the center, which traverses the two grooves. By loosening the screws, we may shorten at pleasure the parts *cG* and *cB* of the two indexes, accordingly as circumstances may require.

The index *AB* having only one point of attachment at *c*, at the center of the circle, moves round this circle, whilst the index *GF* remains constantly in the direction of the diameter, which passes through the points zero and 180 degrees.

It may be proper to remark, that the upper part of the index *AB* ought to be sloped to an edge, on its side *sz*, the direction of which edge, if prolonged, would pass through the center *c* of the instrument. The reason is, that this side is the fiducial edge; that is to say, it indicates upon the graduated circumference the dimensions of the angle we wish to measure.

Mode of operation.

Let us now suppose that we are desirous of measuring in any crystal, the angle which two contiguous planes form with

* This ingenious instrument has been in use a considerable time, and deserves to be more generally known. W. N.

each

DESCRIPTION OF THE GRAPHOMETER.

each other. It is known that this angle is equal to that of two lines drawn to any single point of the edge which unites these planes, provided they be perpendicular with this edge, and situated in the planes themselves. To find this angle, we dispose the instrument in such a manner, that the portions $c G$ and $c B$ of the two indexes shall have no space between themselves and the points in question, and that their sides shall be perpendicular to the edge at which they join. In this case, the faces which embrace the crystal are tangents to the two planes whose incidence we wish to measure. This being done, we look on the circumference of the instrument for the degree marked by the fiducial edge $s z$, or the angle which this line forms with that which passes through the center v and the point zero, which angle is equal to that formed by the two portions $G c$ and $c B$ of the indexes, because vertically opposite.

It is an advantage to be able to shorten these parts at pleasure, in order to avoid the obstacles which would otherwise render the operation impracticable, either from the gangue to which the crystal adheres, or from the contiguous crystals by which it is partly surrounded.

Advantage of shortening the legs.

Cases, however, occur in which this expedient would not be sufficient, and we should find ourselves embarrassed by the part of the semicircle situated towards M , if its position were invariable. The ingenious inventor of the instrument has guarded against this inconvenience by means of the following mechanism :

In some crystals this expedient is not sufficient;

The screw situated at c holds not only the two indexes, but also a steel radius placed below the brass arm, upon which the index $G F$ is immediately applied. The upper extremity of this radius, or that which is situated towards O , has an excavation into which a brass stem enters, which is likewise provided with a screw. The semicircle is besides divided into two pieces, at the place of the 90th degree; so that, by means of a hinge at that part, the quadrant $T M$ may be folded behind the quadrant $T N$, and is, as it were, suppressed. When we wish to perform this movement, we loosen the screw which supported the upper part of the radius $c O$, disengage the excavation which terminates this radius from the screw which was inserted into it, and draw back the radius till it lies beneath the brass arm which supports the index $G F$.

but part of the circular limb must be occasionally removed.

When

When the angle which we measure exceeds 90 degrees, we return the quadrantal arc TM to its former situation, in order to ascertain its dimensions.

The precise measure of angles completes the description of a crystal.

The utility of the goniometer will be very apparent to every one who considers how interesting it is in descriptions of crystals, to know the angles which their faces form with each other. These indications render the description appropriate, by distinctive and truly characteristic traits: without them it can be nothing more than a rude and imperfect sketch, that may apply to a variety of different objects.

Without this they cannot be distinguished.

Thus we do not give a determinate notion of the dodecahedral zircon, when we merely say that it is a prism of four sides, terminated by summits with four rhombuses attached to the longitudinal edges. This description applies equally to the Harmotome (cruciform hyacinth) to the stilbite, to oxidized tin, &c.; but if we add, that the sides form right angles with each other, and that the faces of the summit are inclined towards each other in an angle of $124^{\circ} 12'$, we confine the description to the zircon. If we say that the inclination is $121^{\circ} 57'$, we describe the harmotome; if we say that there are two different inclinations, the one of $123^{\circ} 32'$, and the other of $112^{\circ} 14'$, we describe the stilbite.

Instances.

Other cases.

And still farther: Several varieties of the same substance may present similar forms, differing only in the measures of their angles. Such are, on the one hand, the six rhomboides, and on the other the two decahedrons with rhombic faces, which are found in the carbonates of lime. How shall we describe with accuracy all those varieties, which differ only in the more or less, unless we give the differences with precision? There are even cases in which the use of the goniometer is the only means of avoiding an error, which otherwise would not fail to insinuate itself into the description. Thus the calcareous rhomboid, the angles of which differ only by about $2^{\circ} 18'$ from the right angle, was at first taken for a cube, and we should have continued to denominate it cubic calcareous spar, if geometrical measurement had not rectified this denomination, which is faulty in two respects, both in itself, and with reference to the theory, which demonstrates that the existence of the cube cannot in this instance be reconciled with any of the symmetric laws of diminution.

XI.

Letter of Professor VOLTA to J. C. DELAMETHRIE, on the Galvanic Phenomena.

SIR,

Paris, 18 Vendemiaire, Year 10.

YOU have requested me to give you an account of the experiments by which I demonstrate, in a convincing manner, what I have always maintained, namely, that the pretended agent, or *galvanic fluid*, is nothing but common electrical fluid, and that this fluid is incited and moved by the simple mutual contact of different conductors, particularly the metallic; shewing that two metals of different kinds, connected together, produce already a small quantity of true electricity, the force and kind of which I have determined; that the effects of my new apparatus (which might be termed electromotors), whether consisting of a pile, or in a row of glasses, which have so much excited the attention of philosophers, chemists, and physicians; that these so powerful and marvellous effects are absolutely no more than the sum total of the effects of a series of several similar metallic couples or pairs; and that the chemical phenomena themselves, which are obtained by them, of the decomposition of water and other liquids, the oxidation of metals, &c. are secondary effects; effects, I mean, of this electricity, of this continual current of electrical fluid, which, by the above-mentioned action of the connected metals, establishes itself as soon as we form a communication between the two extremities of the apparatus, by means of a conducting bow; and when once established, maintains itself, and continues as long as the circuit remains interrupted. You have requested this sketch from me, to be inserted in the next number of your *Journal de Physique*, convinced in your own mind of the truth of these observations by some of the experiments which I had the pleasure of shewing you yesterday with my small portable apparatus, in presence of the celebrated philosopher of Geneva, M. Pictet, and some other friends. I regret that I have not sufficient time to enlarge the essay which I send you, so as in some measure to comply with your invitation, and which can answer your expectations only in a partial manner. Accept it then as the precursor of a more ample memoir, which I intend shortly to publish.

General outline of the doctrine of the author respecting galvanism.

That the electrical effects are the primary causes of the oxidation, &c. &c.

Narrative of experiments.

I began by shewing you, by means of experiments, delicate indeed but yet simple, that phenomena unequivocally electrical are produced by the mere contact of two different metals, without the intervention of any wet substance—experiments which ought to be considered as fundamental.

Electrometer and condensers.

In order to render this electricity (which is so feeble that without other artifices it would remain imperceptible) sensible and manifest, I employ my electrometers with fine straws, combined with my condensers, the best of which are those made with two metallic discs, which apply very exactly to each other by their faces, which are very flat, and covered with a slight layer of sealing wax, or, which is better, of good lac varnish.

Insulated discs of copper and zinc are applied together.

The first method of performing this experiment was to take two other discs or plates, the one of copper and the other of zinc; to hold each by an insulating handle (of glass covered with sealing wax); to apply them for an instant to each other by their flat faces, and afterwards separating them dextrously, to bring them into contact with the electrometer, which then indicated, by the divergence of its straws, to the distance of about a line from each other, the electricity which each of the plates had acquired, and whether the electricity was positive (or el. +) in the zinc, and negative (or el. -) in the copper, as could be shewn by approaching a stick of sealing wax, that had been rubbed, to the same electrometer.

On the separation, the zinc was el. +, and the copper el. -.

These plates not only move the electricity, but condense it.

It is proper to observe in this experiment, that the two plates, at the same time that they are *motors of electricity* by virtue of their mutual contact, as different metals, perform also the function of condensers, because they are presented the one to the other by a large surface, in consequence of which their contrary electricities are counterbalanced in the best possible manner. This is the reason why this positive electricity in the plate of zinc, and negative in that of copper, which otherwise would not rise to more than about a sixteenth of a degree, and which in fact does not rise higher as long as these same plates remain applied the one to the other, elevates itself, on detaching them, to 1, $1\frac{1}{2}$, or 2 degrees, and even more.

To increase the electric tension, the discs, when separated, are

This degree of electricity may appear trifling; it does not satisfy some persons, who always wish to see effects exemplified upon a large scale. Be it so. In order to obtain electric

cal phenomena much more distinctly marked, I generally employ a second condenser, mounted upon the electrometer itself, and proceed in the following manner:—I apply the plates of copper and of zinc to each other, and separate them several times, at each separation bringing one of these insulated plates into contact with the upper disc of the condenser, and the other, also insulated, with the lower disc, which is attached to the electrometer. When this contact has been repeated ten, twelve, or twenty times, the disc of the above-mentioned condenser being raised, the electrometer supports the inferior disc alone, which elevates itself to 10, 12, 15, 20 degrees, &c. &c.

repeatedly applied to another condenser.

It might be imagined that, independent of the action of the condenser, the extent of contact between the two different metals greatly contributes, as such, to raise the electricity to the degree which we have seen, and that we should obtain a much inferior degree, if they touched each other only by a few points. But I prove the contrary, that is to say, that in the one case, as well as in the other, the electric tension rises, during the contact, to the same point, which is about a sixtieth part of a degree of my electrometers of thin straws, when the metals are zinc and copper, and a little more when they are zinc and silver; which tension requiring a quantity of electric fluid in the plate which performs the office of condenser, proportionably larger, accordingly as it condenses 60, 150, 200 times, it is evident why we obtain 1, $1\frac{1}{2}$, 2 degrees, &c. &c.

The application of the flat surfaces of the zinc and copper does not increase their motive power.

In order to prove that the contact of two metals of little extent, and which even subsists only at some points, displaces the electric fluid in such a manner as to raise the tension in these metals to the same degree; I join a small plate of copper with another of zinc, either similar or dissimilar with respect to figure and size, applying them to each other at a few points only, or at more points, or even folding them together end to end. See Plate VIII. fig. 3.

Metals of other forms brought into contact by smaller or minute surfaces.

Holding the piece *z* of zinc with two fingers, or in any other manner, I make the other, *c*, of copper, communicate with the superior disc of the condenser, whilst the inferior disc communicates, as it ought, with the ground. A moment afterwards, raising this upper disc into the air, and holding it insulated, it gives me by the electrometer from 2 to 3 degrees

Manipulation. A single couple of pieces, one of which is held in the hand, and the other applied to the condenser, produces

of

electricity; equal to that of the larger contacts; of negative electricity (el. -), accordingly, as such a condenser condenses from 120 to 180 times. This proves that the electric tension of the above-mentioned plate *c* was about a sixtieth of a degree, or nearly equal to that which the two plates of copper and of zinc acquired in the preceding experiments, when applied to each other by the whole extent of their flat surfaces.

which is reversed by touching with the other metal.

When we reverse the experiment, that is to say, when we cause the plate *z* of zinc to communicate with the condenser, we likewise obtain from two to three degrees, but of positive electricity (el. +.)

If the zinc piece touch a copper condenser while the other piece (of copper) is held in the hand, no effect follows;

However, if the disc of the condenser be copper, and the plate *z* touch it immediately without any intervening substance, we obtain nothing, because the zinc being then in contact, at the two opposite ends, with copper and copper, two equal forces act in opposite directions, and by that means destroy or counterbalance each other.

unless moisture be interposed.

It is therefore necessary that the communication of the plate of zinc *z* with the copper disc of the condenser should be effected by the interposition of a conductor, which should be a simple conductor nearly, an humid conductor, such as a piece of wet card or cloth.

But it is asserted, that the humidity acts merely as a conductor, to prevent the opposite metallic contacts in the pile.

As to the rest, the action which excites and gives motion to the electric fluid does not exert itself, as has been erroneously thought, at the contact of the wet substance with the metal, where it exerts so very small an action, that it may be disregarded in comparison with that which takes place, as all my experiments prove, at the place of contact of different metals with each other. Consequently the true element of my electromotive apparatus, of the pile, of cups, and others that may be constructed according to the same principles, is the simple metallic couple, or pair, composed of two different metals, and not a moist substance applied to a metallic one, or inclosed between two different metals, as most philosophers have pretended. The humid strata employed in these complicated apparatus are applied therefore for no other purpose than to effect a mutual communication between all the metallic pairs, each to each, ranged in such a manner as to impel the electric fluid in one direction, or in order to make them communicate, so that there may be no action in a direction contrary to the others.

After

After having well ascertained the degree of electricity which I obtain from one single pair of these metallic pieces, as shewn by the condenser which I employ, I proceed to shew, that with two, three, four pairs, &c. properly arranged, that is to say, disposed all in the same direction, and communicating the one with the other, by as many humid strata (which are necessary, in order that there may be no contrary actions, as I have already shewn), we have exactly the double, triple, quadruple, &c. so that if with a single pair we should be able to electrify the condenser to that point as to make it indicate, by the electrometer, three degrees, for example, we should obtain six with two, nine with three, twelve with four, &c. if not exactly, yet nearly so. You have seen these experiments, and have appeared to be very well satisfied with them, as well as Mr. Pictet, who seemed enchanted with them, and was never tired with seeing them repeated.

The intensity of a succession of couples is the same multiple of that of a single couple of plates.

Here then we have already constructed a small pile, which however does not yet afford any indications, by the electrometer without the aid of the condenser. In order that it may immediately afford these, in order that it may arrive at one whole degree of electric tension, which it will scarcely be possible to distinguish, because it amounts to no more than half a line on the electrometer, it is necessary that such a pile should be composed of about sixty of these combinations of copper and zinc, or, which is better, of silver and zinc, on account of the sixtieth of a degree which each combination yields, as I have already remarked. It then also affords shocks and gives shocks, if we touch its two extremities with wet fingers, and much stronger shocks if we touch them with metals grasped by large surfaces in the hands well moistened, by which means a much better communication is established.

This is the pile;

In this manner we may receive shocks from an apparatus, whether in pile or with cups, of 30 and even of 20 pairs, provided the metals be sufficiently clean, and especially if the humid strata be moistened, not with mere pure water, but with a considerably strong saline solution.

Saline liquids, instead of water, increase the effect;

These saline liquids, however, do not properly augment the electric force;—not at all;—they merely facilitate the passage, and leave a freer course for the electric fluid, being much better conductors than simple water, as several other experiments prove.

but merely, it is asserted, because they are better conductors.

In

Experiment of-
fered in proof.
A series of
glasses, with the
metals and wa-
ter, give a cer-
tain electric in-
tensity and shock;
but on adding
salt to the water,
the intensity is
the same, though
the shock is
much greater.

In order to establish this fact, and render it evident to such as have found a difficulty in admitting it, that the electric force or tension is, if not entirely, yet very nearly the same, whether the wet layers be moistened with pure water or with a saline solution, though the difference in the shock is so great; I have frequently made the following experiments, of which I have spoken to you, and which I would have shewn to you; if I had been provided with the requisite articles. I take 30 cups or drinking glasses, with which I construct one of these apparatuses which I term a crown of cups, putting into them a sufficient quantity of pure water, and causing them to communicate, the first with the second, the second with the third, and so on successively to the last, by means of metallic wires, which terminate at one end in a plate of copper, and at the other in one of zinc, and are all turned in the same direction. The apparatus being constructed in this manner, I try its electric force, by causing the first of the cups to communicate with the ground, and applying the condenser to a piece of metal which is partly immersed in the last: this condenser, when I afterwards withdraw it, and separate one of its discs from the other in the proper manner, and without delay, gives me 40, 60 degrees, and more, according to its condensing force. I also try the shock in the most advantageous manner, and find that it is very slight: after having well ascertained both the degree of the electricity and the weakness of the shock, I add a pinch of salt to each cup, and repeating the proofs, I find that the electricity has not been at all increased, the condenser giving me still only 40 or 60 degrees, as before; but the shocks are incomparably stronger.

The apparatus
charges a large
battery almost
instantaneously.

There are many other experiments, which I have described to you verbally, and which I would willingly have performed before you, had I not been in want of the requisite apparatus. I informed you—at which you were much astonished, and Mr. Pictet still more so—that with one apparatus I charge a Leyden phial, whatever its capacity may be, and even a large battery; that I charge them in an instant, or, to speak more accurately, in less than a twentieth part of a second, and almost to the same degree as the apparatus itself, namely, to about one degree of tension, if the apparatus be composed of 30 pairs; to two degrees if it contain 120, &c.;—that I am then able to draw, by the help of the condenser, a strong spark from

From a small jar charged in this manner, a great number of similar sparks from a large jar, and almost without limit from batteries, as I am actually able to draw them without limit of number from the apparatus itself.

I have informed you, that large bottles charged in this manner gave me moderate shocks, and batteries pretty strong ones, as high as the elbow, and higher; that the shocks of a battery of 10 square feet of coating, and charged in less than a twentieth part of a second by one of my apparatuses of 200 metallic pairs, are very violent and almost insupportable; for I have not yet made any trial with larger batteries; but that there is every probability that the violence of the shocks increases with the size of the batteries, as far as a certain term, which I am not able to define; so that it would be possible, with batteries of 40, 60, and 100 square feet, to give considerably strong shocks, by charging them by the transient contact of a pile only 60, 40, 30, or still fewer pairs.

I have explained to you the manner in which one ought to proceed in order to perform these experiments with success; that it is particularly necessary we should carefully avoid the slightest interruptions in the communications of the conductors with the coatings of the bottles, and between each other, and that this becomes still more necessary when the electromotive apparatus, being composed of a small number of pairs, possesses but little power, so as to be unable to overcome the slightest obstacle that might oppose the passage and the course of the electric fluid.

Lastly, I remarked to you, that these experiments confirm in a very evident manner what all the others already suggested, namely, that the quantity of electric fluid set in motion by my apparatus is much larger for every moment of time than that which is set in motion by the ordinary electrical machines; that the former afford it in much greater abundance than the latter, when the object is to produce, not an accumulation of electric fluid in insulated bodies, in order to raise the electricity to a high degree of tension, which may be done with those machines, but by no means with piles and other similar apparatuses, unless we also employ condensers; but where we require a constant current of this fluid, supported by a continued action of a circle of condensers not insulated; nay, one of my apparatuses, of only 60 or 30 metallic pairs, pours out

Strong shocks from batteries so charged.

Cautions in order to succeed in charging a battery by galvanism.

The pile moves a much greater quantity of electricity than any machine.

in

in every instant, or in any given time, more electric fluid, if it meets with no obstacle, or if the fluid be not obstructed by the too small capacity of the recipient into which it is infused, than one of the most active electrical machines with cylinders or plates of glass. In fact, where shall we find a machine capable of charging a very large battery to one or even half a degree in less than an eighth part of a second, of pouring into it a sufficient quantity of electric fluid to enable us afterwards to draw from it, by the condenser, a great number of sparks in succession, as is done by one of the above-mentioned apparatuses? *

Mention of other experiments.

The other experiments, which I was able to shew you in part, relate to the different electroscopic phenomena which the apparatus presents, accordingly as the one or the other of its extremities communicates with the ground, or both of them, or neither one nor the other, or as they communicate only between themselves and with the ground at the same time; accordingly as these communications are effected by perfect conductors, conductors more or less imperfect, &c. all which circumstances singularly modify and produce great variations in the results, which often appear curious and even anomalous, but which, nevertheless, I think myself able to explain in a satisfactory manner, without deviating from my principles and sound electrical theory, attention being paid to the mode in which the imperfect or bad conductors act. It would carry me too far to enter at present into these details; what you have already seen, and what I have communicated, may be sufficient for the present occasion.

Observations on the preceding Memoir. W. N.

Theory of Volta too hastily adopted.

Sig. Volta, and many of our philosophical neighbours in France, will, no doubt, think, on re-consideration of the facts, that they have been too precipitate in admitting the electric energy as the only effective agent in the phenomena of the pile, and that the fluids act merely as conductors. The late interruption to correspondence may probably have rendered the contents of my late Journal little known in France, otherwise the learned author of this memoir would

* On this subject see Philosoph. Journal, 4to. IV. 243—245. N.

have met a fatal objection to that part of his theory, which gives all to the metals and nothing to the fluids, in * Mr. Davy's galvanic pile with one kind of metal only and three fluids. Davy's galvanic pile, consisting of one metal throughout, but with different strata of fluids: for example thus; metal, cloth soaked in dilute nitrous acid, cloth soaked in water, cloth soaked in sulphuret of potash;—then the same metal, and nitrous acid, and water, and sulphuret;—the metal, &c. Or if a trough be used, the separation between the acid and sulphuret may be made by a plate of horn, and the two fluids may be connected by a slip of wetted paper hung over the edge of the horn, which will not cause the fluids to mix, because water is lighter than either. The metals, separately and successfully tried, were silver, copper, zinc, and lead.

Trough of the same construction.

To this I will here add, from conversation, an experiment of the same philosopher, which is no less conclusive as to the direct efficacy of the fluid in this apparatus; because the same electric power is made to move either from the top or bottom of a pile of two metals, according to the nature of the interposed fluid.—If a pile of copper and iron be constructed as usual, with water interposed, the iron becomes electrified *plus* and the copper *minus*; but if the same, or a similar pile, be constructed with no other difference than that sulphuret of potash is used instead of water, the iron is electrified *minus* and the copper *plus*. In the first case the iron is oxidized; but in the second there is no oxidation of this metal, and the copper is oxidized, and probably sulphurated.

Electric current of the common pile reversed by changing the fluid.

Lastly, we have another instance of the power and importance of the fluids in the article which follows the present, where charcoal alone is used, and the leading condition is, that two different fluids shall be used.

Galvanic series of charcoal only and two fluids.

As we know by the experiments of De Saussure, and many others, that chemical changes do disturb the equilibrium of electricity, and they certainly take place in the pile, it seems at least probable that these may have the chief agency in the apparatus. With regard to the principle of electric motors of Sig. Volta, I must observe, that Bennet made many direct experiments of applying different metals, by the single

Chemical changes are known to produce electricity; and as such changes happen in the pile, they may probably cause its electricity.

* See notice in the Scientific News, Phil. Journal, 4to. for May, 1801, Vol. V. p. 78; and the subject is fully treated in the Philos. Transf. for 1801, in a paper inserted in the same volume of our Journal, 4to. p. 341.

Bennet made ex- and double touch, to the plates of the doubler, followed by
periments on the production of electricity, by him called adhesive electricity,
electricity by the contact of metals which were published in his *New Experiments on Electricity* in
before A. D. 1789, pages 86 to 102; and others were made by Cavallo o
1789.—Cavallo the electricity produced by the contact or stroke of a piece a
before 1795. metal, let fall out of the hand, for the most part, upon an in-
sulated metallic plate, which were published in the third
volume of his *Electricity* in 1795. I do not know the date of
Volta's experiments, but believe them to be much later than

They supposed various electric capacities; but the hypo-thesis of a cur-
rent of electri- city produced by metallic contacts belongs to Volta.
those of the same kind by Bennet. This last philosopher, as
well as Cavallo, appears to think that different bodies have
different attractions or capacities for electricity; but the
singular hypothesis of electromotion, or a perpetual current of
electricity being produced, by the contact of two different
metals is, I apprehend, peculiar to Volta.

XII.

*An Account of a Method of Constructing Simple and Compound
Galvanic Combinations, without the Use of Metallic Substances,
by means of Charcoal and different Fluids. By Mr. DAVY.*

Charcoal be-
tween nitric acid
and water affords
galvanic effect.

1. IF a piece of well burned charcoal be brought in contact
at one of its surfaces with a portion of water, and at another
surface with a portion of nitric acid, a simple galvanic com-
bination will be formed when the two fluids are connected
together. And the powers of it are demonstrated by its agen-
cy upon the limbs of frogs, and by its effects upon the organs
of sense.

Series of such
combinations.

2. A compound galvanic combination or a galvanic battery
may be formed from a number of series composed of the same
substances: but in this case the fluid elements of each series,
not being immediately in contact, must be connected with
similar elements in other series in an order of regular alterna-
tion, such as water, charcoal, acid; water, charcoal, acid;
and so on.

Charcoal battery
by a series of
glasses alternately
charged with
nitric acid and
water, and al-

3. The best mode that has yet occurred of constructing
galvanic batteries with charcoal, is by means of a number of
glasses, which are made to contain, alternately, nitrous acid,
and water, and which are connected in pairs by means of moist-
ened

lined cloth. The pieces of charcoal used are made from very dense wood, such as box, or lignum vitæ; and in this case the fluids will not penetrate into them by capillary attraction, much beyond the places of their primary contact. Their forms are those of arcs, so that each piece connects together two glasses; but in instances where single pieces of charcoal cannot be obtained of the proper shape, two long and thin slips may be fastened together by silk, so as to form the angle necessary to their insertion into the glasses.

ternately connected by charcoal and slips of wet cloth.

4. Twenty series in a battery of this kind produce sensible but feeble shocks, and when a single metallic series with a gold wire and two glasses of water is substituted for one of the primary series, hydrogen is given out by the metallic point in the glass of water in the place of the acid, whilst oxygen is evolved from the point in the other glass.

5. In the galvanic batteries with charcoal, fulphuric acid may be substituted for nitric acid; and solution of sulphuret of potash for the water, without any material alteration in the nature of the agency; the solution of the sulphuret indeed, seems, in some measure, to increase its intensity, and combinations containing this substance, dense charcoal, and concentrated nitric acid, appear to be superior in activity to similar combinations containing copper and the same fluid elements, and to be nearly equal to those composed of zinc, silver, and water.

Other fluids.

January 9, 1802.

XIII.

Short Analysis of the Principles and Structure of Mr. CLOSE's Hydraulic Engine.—W. N.

AN eminent engineer, to whose talents and invention society is highly indebted, writes to me that he cannot understand Mr. Close's engines, and requests some explanation, as he does not conceive that impossibilities would have my sanction. Though I am convinced that an attentive revival of the descriptions would render my elucidation unnecessary, yet as that which he requests may not be unacceptable to other readers, I have here annexed a few observations; viz.

An explanation of Mr. Close's engines requested.

VOL. I.—FEBRUARY.

L

1. That

Short enumeration of its principles and mode of operation.

1. That the water (of which part is intended to be raised above the level) must have a fall. 2. That this fall affords the usual difference between the legs of the syphon. 3. That if the syphon had two or more ascending legs, water would rise through them all. 4. That if one of these legs were small and contained air, that air would pass into the great syphon, and if the velocity of the fluid were rapid enough, the bubbles would pass down along with the water, instead of rising to the top and breaking the column. 5. That if there were a contrivance of two cocks in the water way of the small ascending leg, and both were shut, the portion of water might be drawn out from between them by a side cock, and that space filled with air. 6. That this space may, if thought fit, be a globe or other vessel. 7. That this space being above the level, the water thus drawn off *is in fact raised*. 8. That by shutting the side cock, and opening the two others, the included air would ascend and be carried down the great syphon. 9. After which another like portion of water might be drawn out as before, &c. &c. And, 10. It is clear that by small syphon work the cocks might be opened and shut without attendance.

The water is raised as usual by its fall.

Utility of publishing new inventions.

This is only one of the many methods of raising water above its level by virtue of its fall. It certainly possesses merit and originality of invention; but whether it be generally preferable to other known methods is another question. For my part, I think it is of advantage to our general stock of knowledge and invention, to publish all ingenious novelties, without any particular solicitude respecting the extent of their power. A variety of circumstances and local situations afford opportunities of applying engines, or their parts, with singular advantage, which in the general contemplation of their structure and use, might seem to be more striking for their singularity than their effect.

XIV.

Simple Process for taking a Copy of a recent Manuscript. Communicated to the Philomathic Society at Paris by CHARLES COQUEBERT.

THIS process is the more interesting as it requires neither machine nor preparation; and may be used in any situation. It consists in putting a little sugar in common writing ink, and with this the writing is made on common paper sized as usual, when a copy is required; this unsized paper is taken and lightly moistened with a sponge. The wet paper is then applied to the writing, and a flat iron, such as is used by laundresses, of a moderate heat, being lightly passed over the unsized paper, the counter-proof or copy is immediately produced*.

Sugar is put in the ink. Damp unsized paper is laid on the writing. The impression or copy is made by an heated iron.

XV.

Narrative and Explanation of the Appearance of Phantoms and other Figures in the Exhibition of the Phantasmagoria. With Remarks on the Philosophical Use of common Occurrences.—
W. N.

A VERY striking application of the magic lantern has been made this winter to the public amusement by M. Philipsthal at the Lyceum. The novelty consists in placing the lantern on the opposite side of the screen which receives the images, instead of on the same side as the spectator, and suffering no light to appear but what passes through, and tends to form those images. His slides are therefore perfectly opaque, except that portion upon which the transparent figures are drawn, and the exhibition is thus conducted.

Phantasmagoria. New application of the magic lantern by placing it on the opposite side of the screen, and making the figures alone transparent.

* I have not heard who was the original inventor of this useful process. That sugar prevents ink from speedily drying has been long known, and this method of impression has been used in some of our public offices, and elsewhere in England for some years.

W. N.

Account of the
exhibition of
Mr. Philipsthal.

All the lights of the small theatre of exhibition were removed, except one hanging lamp, which could be drawn up so that its flame should be perfectly enveloped in a cylindrical chimney, or opaque shade. In this gloomy and wavering light the curtain was drawn up, and presented to the spectator a cave or place exhibiting skeletons, and other figures of terror, in relief, and painted on the sides or walls. After a short interval the lamp was drawn up, and the audience were in total darkness, succeeded by thunder and lightning; which last appearance was formed by the magic lanthorn upon a thin cloth or screen, let down after the disappearance of the light, and consequently unknown to most of the spectators.

Darkness.
Thunder.
Lightning.

Figures of the
departed; phan-
toms, &c.

These appearances were followed by figures of departed men, ghosts, skeletons, transmutations, &c. produced on the screen by the magic lanthorn on the other side, and moving their eyes, mouth, &c. by the well known contrivance of two or more sliders. The transformations are effected by moving the adjusting tube of the lanthorn out of focus, and changing the slider during the moment of the confused appearance.

The figures
seem to recede
to an immense
distance.

It must be again remarked, that these figures appear without any surrounding circle of illumination, and that the spectators, having no previous view or knowledge of the screen, nor any visible object of comparison, are each left to imagine the distance according to their respective fancy. After a very short time of exhibiting the first figure, it was seen to contract gradually in all its dimensions, until it became extremely small and then vanished. This effect, as may easily be imagined, is produced by bringing the lanthorn nearer and nearer the screen, taking care at the same time to preserve the distinctness, and at last closing the aperture altogether: and the process being (except as to brightness) exactly the same as happens when visible objects become more remote, the mind is irresistably led to consider the figures as if they were receding to an immense distance.

How effected.

Transforma-
tions.

Several figures of celebrated men were thus exhibited with some transformations; such as the head of Dr. Franklin being converted into a skull, and these were succeeded by phantoms, skeletons, and various terrific figures, which instead of seeming to recede and then vanish, were (by enlargement) made suddenly to advance; to the surprize and astonishment of the audience, and then disappear by seeming to sink into the ground.

Skeletons, &c.
suddenly rush
forward.

This

This part of the exhibition, which by the agitation of the spectators appeared to be much the most impressive, had less effect with me than the receding of the figures; doubtless because it was more easy for me to imagine the screen to be withdrawn than brought forward. But among the young people who were with me the judgments were various. Some thought they could have touched the figures, others had a different notion of their distance, and a few apprehended that they had not advanced beyond the first row of the audience.

The deception not equally effectual to all spectators.

As I have given this account, of an exhibition on which an ingenious mechanic in part depends for his support, it will not be impertinent to my present and future readers to add, that the whole, as well as certain mechanical inventions, were managed with dexterity and address, and that his gains in London have been very considerable. The figures for the most part are but poorly drawn, and the attempt to explain the rational object, or purpose of the exhibition was certainly well intended; but unfortunately for the audience his English was unintelligible. His lightning too, being produced by the camera was tame, and had not the brisk transient appearance of the lightning at the theatres, which is produced by rosin, or lycopodium powder, thrown through a light, which in Mr. P's utter darkness might easily have been concealed in a kind of dark lanthorn.

Observations on the performance.

Lightning.

My young pupils on their return made drawings, and applied the magic lanthorn to a sheet in a door way between two rooms. Some of their drawings were made on thin paper and varnished, to render them transparent, and others were on glass. The paper figures were less bright than the others; but an advantage may be had in this material by those who cannot draw, because they may colour and varnish small figures, engraved in aqua-tinta or in any other manner without stroke.

Imitation by some young persons.

A plate of thin sheet iron, such as German stoves are made of, is an excellent instrument for producing the noise of thunder. It may be three or four feet long, and the usual width. When this plate is held between the finger and thumb by one corner, and suffered to hang at liberty, if the hand be then moved or shaken horizontally, so as to agitate the corner at right angles to the surface, a great variety of sounds will be produced; from the low rumbling swell of distant thunder, to the succession of loud explosive bursts of thunder from elevated clouds,

Remarkable imitation of thunder by a sheet of iron

clouds. This simple instrument is very manageable, so that the operator soon feels his power of producing whatever character of sound he may desire; and notwithstanding this description may seem extravagant, whoever tries it for the first time will be surprized at the resemblance. If the plate be too small, the sound will be short, acute, and metallic.

To Correspondents.

Ludicrous Essay.

Our best philosophers have profited most from common occurrences.

Ridicule not injurious to the progress of philosophy.

The present seems to be no unfit place to notice some correspondents which my late paper on shaving has induced to favour me with a few letters. To those who find themselves instructed and gratified by a small addition to their daily comforts, I can only give my congratulation; but to that friend who calls himself a *Shaver*, but who I fear is no true shaver, from the little veneration he seems to have for the art, I am to return my thanks for his *Essay on the Art of cutting Bread and Butter*, and must say, that if it had abounded with instruction equal to its merriment, I should have been glad to have given it to my readers. He who shall invent a machine to perform even this operation, will, I think, deserve well of society. I would humbly propose it as my opinion, not without expectation that many others may think with me, that no subject is beneath the consideration of a philosopher. Our best philosophers have been most studious of the daily occurrences of life. Newton's attention was attracted by the fall of an apple before he extended the theory of gravitation to the moon. Soap bubbles and the prism were play-things before he selected them as instruments to analyse the rays of light. Franklin by the kite of a child conducted lightning from the clouds to the earth: and in a word, it appears that the greatest discoveries have been made, not by those who could command the expensive and ornamental apparatus of showy experiment, but by such as were in the habit of close attention to the means, the agents, and the operations which are constantly performed around us, and frequently ill understood because habitually neglected.

But laughter is said to be one of the distinctive characters of the human species, and our pleasures are not so many that we should reject any innocent source of amusement. No serious evil is to be apprehended from raillery, especially if attended

tended with good nature. Herschel will not be diverted from pursuing his discoveries, because the inhabitants of Laputa give themselves too much anxiety about the sun's good health. Let the man of wit enjoy his joke, and the man of experiment his rational toys, provided both unite in cultivating that amiable spirit of philanthropy, on which the happiness of social intercourse so much depends.

SCIENTIFIC NEWS.

Extract of a Letter from Dr. LORENZ DE CRELL. (Helmstadt, Dec. 14, 1801.) On the Chlorophane and a supposed Variety of Barytes.

THE Prince of Gallitzin, F.R.S. possessor of a most excellent mineralogical cabinet, and living now as a private individual at Brunswick, has observed that the chlorophane, or that Siberian fluss-spar, which, after being gently heated on charcoal, turns green (on account of which it is called pyrosmaragdus in the *Chemische Annalen*) and on being cooled regains its former colour; that the chlorophane, I say, lost this property on being exposed in a tin box to the rays of the sun in a window during the summer. It was turned quite colourless, and exhibited no light at all on being heated.—The Count of Hoffmansegg, whose travels to Portugal (for discoveries in natural history) were written by Professor Linck, his companion, has brought with him two stones, greatly resembling quartz, or rock crystal externally, but without regular figure; giving sparks with steel, but of far greater specific gravity, viz. 3,500. It shewed some electricity on friction. This new kind of stone (for such it is thought by good mineralogists) is to be analysed chemically. In the mean time Dr. Bruckman, who is well known by his books on precious stones, thinks proper to call it *barytes nobilis*.

The chlorophane loses its luminous property by exposure to light.

Barytes nobilis.

Corrindon or Adamantine Spar found near Philadelphia.

M. Guillemard, a learned mineralogist, who has travelled much in North America, writes to Dr. Delam  therie, that Mr. Adam Seybert has discovered corrindon or adamantine spar

Corrindon or adamantine spar found in America.

spar at Chestnut-hill, about ten miles from Philadelphia. It is known that the sea coast in these parts is entirely granitic, and this granite extends to a considerable distance inland. Farther inland succeeds the gneiss; then micaceous schistus; then grit stone; and, lastly, the ground towards the lakes Ontario and Erie is all calcareous.

The granites of the coast of Philadelphia contain various marked veins of granite, as is observed in different granitic soils.

It was in one of these granitic veins, which may be almost compared to loads (filons), that Mr. Seybert found corindon mixed with the other substances or elements of granite which compose the vein.

J. de Phys. Brum. 10.

Nature of the Earth which is eaten by the Inhabitants of New Caledonia, ascertained by CIT. VAUQUELIN.

Savages on the Oronoko eat earth, which is thought to be nourishing.

Humboldt, in a letter to Fourcroy, of which an extract was given in the 50th number of the Bulletin des Sciences, affirms, that the Otomagues almost entirely subsist on a kind of earth for three months, when the Oronoko is so high that they can find no more turtle. Some of them eat a pound and a half daily. It is affirmed by the missionaries that they mix it with the fat of the crocodile's tail; but Mr. H. insists that they do nothing but slightly burn and moisten it. He thinks that moistened earth may be nourishing by decomposing the air, or by some effect of the chemical affinities.

Inhabitants of New Caledonia also eat earth.

Cit. Labillardiere has stated a fact equally singular, from an observation made in a part of the world considerably distant from that inhabited by the Otomagues; that the inhabitants of New Caledonia, when pressed by hunger, eat a considerably large quantity of a greenish steatites, which is soft and friable. It may be easily imagined how the dreadful custom of eating prisoners of war might have been introduced amongst a savage people, who are reduced to such want as to be obliged to satisfy their hunger by distending their stomach and intestines with an earthy substance, which has no other alimentary quality but that of being light and friable.

Cit.

Cit. Vauquelin, wishing to know the nature of this earth, This, by analysis, found to contain no nutritive parts, has analysed it by the usual methods, from some specimens which were sent him by Cit. Labillardiere.

This earth is soft to touch, composed of small fibres, which are easily separated, and when ignited it loses $\frac{1}{100}$ of its weight. It is composed of,

37 parts of pure magnesia,

36 of silica.

17 of oxide.

3 or 4 of water.

2 or 3 of lime and copper.

It does not therefore contain any alimentary parts, and can only be considered as filling the stomach; a kind of mechanical expedient to suspend the pains of hunger.

Soc. Philomath. No. 55, An. 10.

Account of a blue Oxide of Iron. By CIT. VAUQUELIN.

This substance was sent to the Counsel of Mines, by the Baron de Molt. It is of a light blue colour, and has the form of small insulated masses in the cavities or clefts of quartz and hard green steatites. It is friable, but rather greasy to touch, and changes its colour before the blow-pipe, and at last melts into a greenish white glass. Blue oxide of iron not yet analysed.

Neither acids or weak alkalis alter its colour. These circumstances distinguish it from lapis lazuli and the prussiate of iron.

This substance communicates a saffron yellow to the muriatic acid in which it was digested, and changes its own colour a little, but cannot be discoloured unless it be at the same time dissolved. There only remains a small quantity of silica, which appears to have constituted its gangue.

On examining the muriatic acid that was used for this operation, it was found it had taken up alumina, lime, and oxide of iron, but neither sulphurated hydrogen, manganese, nor phosphoric acid were discovered in it, substances to which the blue colour of this oxide of iron might be ascribed. It therefore remains to be determined what is the cause of the remarkable colour of this oxide, a colour which has never yet been

been given to this metal by any chemical process. It appears only that the iron in this oxide is carried to a degree of oxygenation nearly approaching to the *maximum*.

Bulletin de Sc. No. 55, An. 10.

Use of Whiskers in certain Quadrupeds. By M. VROLYK, Professor of Natural History at Amsterdam.

The whiskers of animals appear to be useful to feel their way in irregular cavities.

This naturalist has endeavoured to ascertain, by experiment, of what use the long stiff hairs, termed *whiskers*, which are placed near the mouth of certain quadrupeds, might be to them. Having disposed a number of books, placed on their edges, upon the ground in his chamber, so as to form a kind of labyrinth, he let loose a rabbit, with his eyes bound up, amongst them. The animal succeeded in extricating himself from this labyrinth, without having overturned them; but when Mr. Vrolyk had cut off his whiskers, the animal, having no longer these tentacula to direct him, ran against the books, and overturned them. Moreover it is known that the bulb, in which each of the whiskers is implanted, receives a small nervous filamen proceeding from the infra-orbital nerve.

Soc. Philom. No. 50, X.

Galvanic Charge of a large Battery by the Metallic Pile. By Dr. VAN MARUM.

The Harlem battery charged by galvanism.

Dr. Van Marum, of Harlem, has charged 25 large electric jars (containing $137\frac{1}{2}$ square feet of coating) by an almost instantaneous contact of a galvanic pile of 200 pair of 3 Guilder silver pieces, alternated with zinc and moist cloth. The electric intensity was equal to that of the pile itself, but the shock much less. The shock of the battery was generally equal to that of half the number of pairs employed in its charge. It was, however, very strong, and reached up to the shoulders. As the 120th Number of the *Annales de Chimie*, in which his letter is inserted, arrived late in the month, I must defer the rest of the account.

ACCOUNT

ACCOUNT OF BOOKS OF SCIENCE.

A Syllabus of a Course of Lectures on Chemistry, delivered at the Royal Institution of Great Britain. By Mr. Davy. 90 pages, 8vo. Cadell, London.—On Thursdays and Saturdays at two, and on Thursdays at eight o'clock, P.M. The morning lectures are read on general chemistry, and the evening lectures on the connection of chemistry with the arts.

The present course is divided into three parts. Part I. *Davy's chemical lectures.* The chemistry of ponderable substances; under six divisions.

1. Of the chemical powers, and the modes of their application.
2. Of undecomposed substances, or simple principles.
3. Of bodies composed of two simple substances.
4. Of bodies composed of more than two simple substances.
5. Of substances composed of different compound bodies, or of compound bodies and simple bodies.
6. Of the general phenomena of chemical action.

Part II. The chemistry of imponderable substances; under four divisions.

1. Of heat or caloric.
2. Of light.
3. Of the electrical influence.
4. Of galvanism.

Part III. The chemistry of the arts; under eight divisions.

1. Of agriculture.
2. Of tanning.
3. Of bleaching.
4. Of dyeing.
5. Of metallurgy.
6. Of the manufactory of glass and porcelain.
7. Of the preparation of food and drink.
8. Of the management of heat and light artificially produced.

It is unnecessary to make any general remarks on the benefits derived to science from correct and sufficiently extensive outlines of facts and doctrines published from time to time by their active cultivators. The present work must prove eminently useful to the pupils of the lecturer, and will add to his reputation by its perspicuity and order. In the rapid progress of experimental discovery, even those learned men who attend to the general state of natural philosophy, without closely following its minuter variations, will be essentially served by clear notices of the present state of one of its chief departments.

The

The Elements of Book-keeping : comprizing a System of Merchants' Accounts, founded on real Buſineſs, and arranged according to Modern Practice. By P. Kelly, Maſter of Finsbury Square Academy. Price 5s. Johnson, Robinfons and Rivingtons.

Kelly's Elements of Book-keeping.

The improvements which time and experience have effected of book-keeping, do not comprehend any change in the original principle of double entry; but in the arrangement and claſſification of ſimilar accounts, which facilitate the operations of commerce nearly in the ſame manner as the buſineſs of manufactures is expedited by the diviſion of labour. The elementary treatiſe before us exhibits theſe improvements.

The author introduces his work by a concise account of the hiſtory of this art, which he thus concludes: "from the foregoing view of the principal authors who have written on book-keeping, it appears that they have been compoſed of two different deſcriptions, poſſeſſing very diſtinct qualifications. The firſt, and by far the moſt numerous claſs, were teachers who have explained the principles, without advertiſing to the progreſſive improvements of practice; and the ſecond, merchants who have exhibited thoſe improvements, without explaining the principles. The productions of both claſſes of writers are highly uſeful, and to combine their utility is the object of the preſent undertaking."

This work comprizes three ſets of books, the firſt explains and illuſtrates the principles of both ſingle and double entry; the ſecond is a further exemplification of the Italian method of double entry by waſte book, journal, and ledger, which does not differ from the ſystems given by other authors; but the third ſet of books differ eſſentially, and therefore deſerves particular notice. Here the waſte-book is divided into a number of ſubſidiary books, each of which is the regiſter of its peculiar portion or department of buſineſs, and each book is divided into monthly tranſactions. By this means the journal is greatly ſhortened and ſimplified; but the principal advantage of this ſyſtematic arrangement conſiſts in poſting to the ledger; for there a whole month's caſh, bills, commiſſion, inſurance, or intereſt, are each carried in one ſum or entry from the journal to the ledger. This method, by which repetitions are avoided, and labour conſiderably diminiſhed, is now generally adopted in many principal mercantile houſes. The author informs

informs us in his preface, that " he has had access, for many years, to the books of several eminent merchants; and that he has endeavoured in all cases to follow the most approved precedents of mercantile experience."

In the third set of books the invoices, sales, and other transactions, have been selected from merchants books:—the advantage of founding a system of book-keeping on real occurrences are obvious; for although the principle of double entry can be explained by fictitious examples, yet these may give the learner wrong notions of business; whereas from real transactions, he will obtain so much practical information of commercial affairs in general, as must interest the mind, and more effectually fix the attention in the course of study.

Memoirs sur l'Influence de l'Air, &c.—Memoirs concerning the Influence of the Air, and several gaseous Substances, on the Germination of various Kinds of Grain. By Huber and Senebier, 1 Vol. in 8vo. Geneva. Palchoud, 1801.

This work is curious from the circumstances under which it was composed. Cit. Huber, already well known for his writings on Bees, is blind, notwithstanding which he has performed those experiments which were suggested to him by C. Senebier. The experiments were made in order to determine the influence of various gases, particularly the oxygen gas on germination*. The seeds were placed either on wet flannels or sponges under receivers filled with gas; the principal results were as follows. All the grains placed in the azotic gas remained unaltered, but germinated on being afterwards placed in the open air. Their growth was accelerated, though slightly, in pure oxygen, but was more vigorous in that which contained a little carbonic acid. In this experiment the carbon of the grain combines with the oxygen, and forms carbonic acid gas. The seeds sprung up in artificial atmospheric air, the same as they would have done in common air. The proportions most favourable for germination, are three measures of azote or hydrogen to one of oxygen. The grains which did not grow when included

Huber and Senebier on the influence of air on vegetation.

* On this subject see also Cruickshank, in the Phil. Journal, 4to, Vol. I. page 339, for Nov. 1797.

Huber and Sen-
nebier on the
influence of air
in vegetation:

in azote, were not affected by a proper quantity of oxygen being introduced by degrees, but shot forth very well when the same quantity was introduced at once. This difference may be explained by considering, that in the first case the oxygen is successively employed in receiving from the grain the carbon which is disengaged, so that none remains to vivify it, but when it is poured in at once, there is sufficient for both purposes.

Seeds do not germinate either in carbonic gas, or pure hydrogen gas. One lettuce seed absorbs in order to germinate a portion of oxygen, which is equal to 26 milligrammes of water, (half a grain) and it does not grow unless the oxygen is at least the eighth part of the atmosphere in which it vegetates. A great quantity of carbonic acid gas is more injurious to germination than the azote, and the azotic gas more so than hydrogen.

If seeds be put to grow in hydrogen gas, the carbon of the grains dissolves in it, and combines very intimately.

The vapour of sulphuric ether placed under a recipient of atmospheric air, prevented the seeds from growing, without altering the quantity of oxygen in the air. The same happens with camphor, oil of turpentine, assa-fœtida, vinegar and ammonia. Putrefied bodies prevent germination, by the abundance of carbonic acid gas they emit. It appears from the preceding facts that oxygen is indispensibly necessary to germination, and that it serves to carry off from the grain that carbon which is disengaged by fermentation. But this rule is not without its exceptions.

In fact peas have germinated in water deprived of air by every possible means, and at whatever depth they have been immersed. The seeds of beans, lentilles, spinach, lettuce and corn, grow in the same manner beneath the water with greater or less facility. These seeds germinate better in water charged with oxygen gas, than in water entirely deprived of it, but not at all in water charged with carbonic acid. The acids retard their germination. The air emitted by peas under pure water, is a mixture of carbonic acid and carbonated hydrogen.

Peas have germinated in pure hydrogen gas and in air, where other seeds have already germinated, and they have totally exhausted the oxygen it contained. In this experiment the hydrogen gas becomes loaded with carbon. They like-
wise

wife germinated in azote gas. They do not germinate in oil; but if after having been swelled in water they are put into oil, they germinate very well.

These facts afford new inductions in favour of the decomposition of water in germination, and consequently in vegetation.

Bull. des Sc. x. No. 55.

Mémoires d'Agriculture, &c.—p. 79.

At the head of this volume are the order of the prefect for printing it, the rules of the Society, and a list of its ordinary and corresponding members, with an account of the Society's labours: after which are the following memoirs.—Instructions for preserving wheat from smut: by CC. Cadet-de-Vaux, Parmentier, Saint-Genis, and Yvart.—Report of the experiments of C. Houdart, junior, on preparing and economising feed.—On the means of draining various lands by simple and not expensive processes, by C. Chaffiron. This memoir, which has been published separately, is preceded by some observations on the general system of inland navigation in France, and followed by a comparative table of the coal-mines worked in each department, and of those which require only navigable canals and rivers to enable them to be worked. The former are fifty-one in number; the latter, sixty. Their importance to those of the French manufactures that require fuel, or mechanic arts, and agriculture, must be obvious, at a time when they are perhaps not sufficiently aware of the want of wood, with which they are threatened, from the great devastation of forests in most parts of the republic; a subject which has been treated of by C. Lasterie in a separate memoir in this volume.—On the precise signification of the terms *agriculture* and *rural economy*, by C. Cels.—Some reflections on the supposed number of sheep in France, by C. Delong. The intention of this memoir is to excite inquiry concerning this important subject.—The advantages of nurseries on estates of a certain extent, in facilitating such annual plantations as may be suited to them, by C. Villele.—An interesting account of the successful amputation of the fore-leg of a cow, which had been fractured, by C. Chaumontel.—Experiments, inquiries, and observations on elms, by C. Boucher; to which C. Denongelles has added his experiments for obtaining alcohol from the sap of vegetables.—On the product of different sorts of wheat that are cultivated, and the melioration of them; or the advantages that would result from inquiring what sorts of

Memoirs of the
Agricultural Society of the department of the
Seine.

wheat

wheat yield the most bread; by C. Chancey.—On rural edifices, by C. Garnier-Deschênes.—On the manner in which the mountains in the Cevennes are fertilized, by C. Chaptal.—On the means of rendering fit for use wells that have been abandoned in consequence of the mephitization of the soil, by C. Cadet-de-Vaux.—Observations on the working of mines, by C. Creusé-Latouche.—On the cultivation of the sugar-cane in the Caribbee islands, and particularly of that of Otaheite by C. Moreau-St.-Mery.—On the cultivation of the sugar-cane, by C. Coffigny.—On the cinnamon-tree of French Guiana, by C. le Blond.—The volume concludes with accounts of the lives of Cretté de Palluel, and T. Francis de Grace.

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HISTOIRE Naturelle des Oiseaux de Paradis, par Viellot, No. 13, 1l. 15s,

Dictionnaire raisonné & universel des Arts & Métiers, par Jaubert, 1801. 5 Vol. 8vo. 1l. 11s. 6d.

Manuel du Voyageur à Paris, An. X. 2s. 6d.

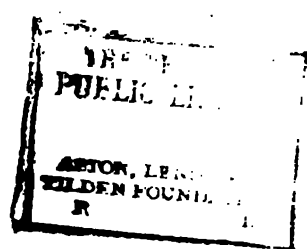
Voyage pittoresque & physico-économique dans le Jurat, par Lequinio, An. ix. 2 Vol. 12s.

Géographie, moderne & universelle, par Lacroix, précédé d'une Traite de la Sphere & d'un d'Astronomie, 1800, 2 Vol. 8vo. avec Cartes, 18s.

Nouveau Dictionnaire François-Italien, & Italien-François, Abregé d'Alberti, 1801. 2 Vol. 8s.

Respecting agri-
cultural infor-
mation.

In answer to the obliging letter of *A friend to Agriculture*, recommending that subject to constitute part of the present Journal, I am to observe, that though this most interesting department of human industry and research is of the first importance to society, and its processes are scientific throughout, yet from the complicated number of causes to be developed, the considerable time required for experiment, the large scale of operation, the uncertainty of reporters, and other considerations, I have been less forward to press this object, than from its high value I should else have been. Communications, selections, or accounts, which from their authenticity and their decisive effect, may tend to establish points of essential value, will undoubtedly conduce to the purposes of our publication; and in this respect I shall thankfully avail myself of the liberal offer of my correspondent.



Bleaching of Paper Stuff

Fig. 3.



Fig. 2.

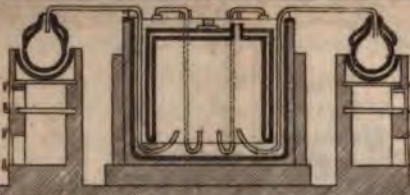


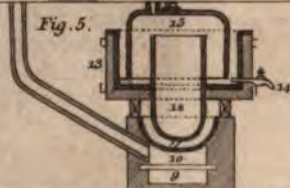
Fig. 1.



Fig. 4.



Fig. 5.



1 2 3 Metres

The Graphometer of Carangeau.

Fig. 1.



D.^r Thompson's Experiments

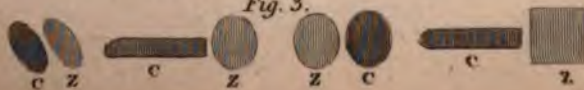
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thrown into Currents
by Heat.*

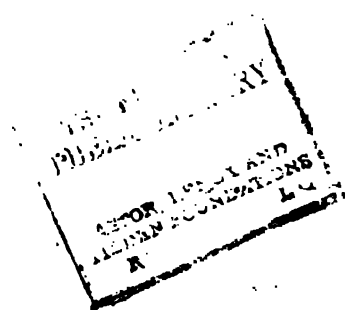
Fig. 2.

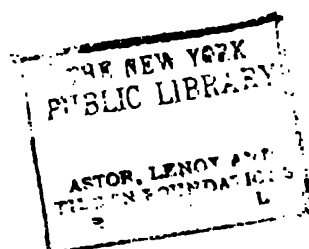


Volta's Theory of Galvanism.

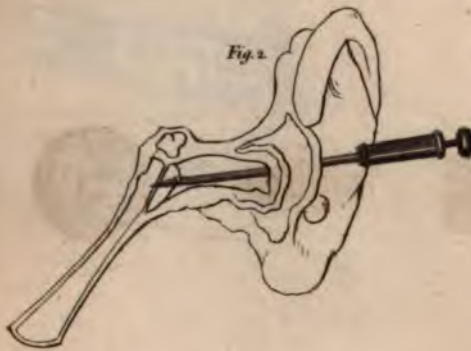
Fig. 3.







Cure of Deafness by puncture of the Meml. tympani.



Manufacture of Gunflints.



Fig. 1.



Fig. 2.



Fig. 3.



Fig. 4.



Fig. 5.



Fig. 6.



Fig. 7.



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BOOK AND
OPERATIONS
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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MARCH, 1802.

ARTICLE I.

Description of an Engine which operates by the Pressure and Descent of a Column of Water against a Piston; nearly in the same Manner as the double Steam Engine operates by Means of Steam. Communicated by the Inventor Mr. RICHARD TREVITHACK, of Camborne, near Truro, in Cornwall.

IN a late conversation with the inventor of the engine which forms the subject of the present memoir, the method of applying a column of water as a first mover, by its alternate action upon a piston in a cylinder, was mentioned by me as likely to prove advantageous in a variety of situations, and probably to afford as beneficial a result with regard to power, as any of the other ways of applying a fall of that fluid. It was with no small gratification, that I found this skilful engineer had amply considered the subject, and had carried it into full effect. I immediately saw, that the communication of results so interesting and authentic, would be highly acceptable to my readers, and honourable to the Journal. Upon my request, he with great readiness and liberality supplied me with the drawing exhibited in Plate IX. and had the goodness to dictate the materials, whence the following description was made.

Vol. I.—MARCH.

M

The

History of a new engine for raising water by an inclosed column.

Description of the parts, and their organization.

The engine, of which a section is here given on a scale of half an inch to a foot, was erected three years ago at the Druid Copper Mine, in the parish of Illogan, near Truro, in Cornwall.

It was worked about two years; but the mine has not proved sufficiently beneficial to encourage the proprietors to go on during the last twelvemonth. A B represents a pipe six inches in diameter, through which water descends from the head to the place of its delivery to run off by an adit at S, through a fall of 34 fathom in the whole; that is to say, in a close pipe down the slope of a hill 200 fathoms long, with 26 fathoms fall; then perpendicularly six fathoms, till it arrives at B, and thence through the engine from B to S two fathoms. At the turn B the water enters into a chamber C, the lower part of which terminates in two brass cylinders four inches in diameter; in which two plugs, or pistons of lead D and E, are capable of moving up and down by their piston rods, which pass through a close packing above, and are attached to the extremities of a chain leading over, and properly attached to the wheel Q, so that it cannot slip.

The leaden pieces D and E are cast in their places, and have no packing whatever. They move very easily; and if at any time they should become loose, they may be spread out by a few blows with a proper instrument, without taking them out of their place. On the sides of the two brass cylinders, in which D and E move, there are square holes communicating towards F and G, which is an horizontal trunk or square pipe, four inches wide and three inches deep. All the other pipes G, G and R are six inches in diameter, except the principal cylinder wherein the piston H moves; and this cylinder is ten inches in diameter, and admits a nine foot stroke, though to accommodate the drawing to our Journal, it is here delineated as if the stroke were only three foot.

The piston rod works through a stuffing box above, and is attached to M N, which is the pit-rod, or a perpendicular piece divided into two, so as to allow its alternate motion up and down and leave a space between, without touching the fixed apparatus or great cylinder. The pit-rod is prolonged down into the mine, where it is employed to work the pumps, or if the engine were applied to mill work, or any other use, this rod would form the communication of the first mover.

K L,

K L, is a tumbler or tumbling bob, capable of being moved on the gudgeons V, from its present position to another, in which the weight L shall hang over with the same inclination on the opposite side of the perpendicular, and consequently the end K will then be as much elevated as it is now depressed.

New engine for raising water by the descent of a column inclosed in a pipe.

The pipe R S has its lower end immersed in a cistern, by which means it delivers its water without the possibility of the external air introducing itself; so that it constitutes a torricellian column, or water barometer, and renders the whole column from A to S effectual, as we shall see in our view of the operation.

The operation. Let us suppose the lower bar KX of the tumbler to be horizontal, and the rod FO so situated, as that the plugs or leaden pistons D and E shall lie opposite to each other, and stop the water ways G and F. In this state of the engine, though each of these pistons is pressed by a force equivalent to more than a thousand pounds, they will remain motionless, because these actions being contrary to each other, they are constantly in equilibrio. The great piston H being here shewn as at the bottom of its cylinder, the tumbler is to be thrown by hand into the position here delineated. Its action upon O F, and consequently upon the wheel Q, draws up the plug D, and depresses E, so that the water way G becomes open from A B, and that of F to the pipe R: the water consequently descends from A to C; thence to G G G, until it acts beneath the piston H. This pressure raises the piston, and if there be any water above the piston, it causes it to rise and pass through F into R. During the rise of the piston, (which carries the pit-rod M N along with it) a sliding block of wood I, fixed to this rod, is brought into contact with the tail K of the tumbler, and raises it to the horizontal position, beyond which it oversets by the acquired motion of the weight L.

Operation, or working process.

The mere rise of the piston, if there were no additional motion in the tumbler, would only bring the two plugs D and E to the position of rest, namely to close G and F, and then the engine would stop; but the fall of the tumbler carries the plug D downwards quite clear of the hole F, and the other plug E, upwards quite clear of the hole G. These motions require no consumption of power, because the plugs are in equilibrio, as was just observed.

New engine for
raising water by
the descent of a
column included
in a pipe.

In this new situation the column A B no longer communicates with G, but acts through F upon the upper part of the piston H, and depresses it; while the contents of the great cylinder beneath that piston are driven out through G G G, and pass through the opening at E into R. It may be observed, that the column which acts against the piston is assisted by the pressure of the atmosphere, rendered active by the column of water hanging in R, to which that assisting pressure is equivalent, as has already been noticed.

When the piston has descended through a certain length, the slide or block, at T, upon the pit-rod, applies against the tail K of the tumbler, which it depresses, and again oversets; producing once more the position of the plugs D E, here delineated, and the consequent ascent of the great piston H as before described. The ascent produces its former effect on the tumbler and plugs; and in this manner it is evident that the alternations will go on without limit: or until the manager shall think fit to place the tumbler and plugs D E in the positions of rest; namely so as to stop the passages F and G.

The length of the stroke may be varied by altering the positions of the pieces T and I, which will shorten the stroke the nearer they are together; as in that case they will sooner alternate upon the tail K.

As the sudden stoppage of the descent of the column A B, at the instant when the two plugs were both in the water way, might jar and shake the apparatus, those plugs are made half an inch shorter than the depth of the side holes; so that in that case the water can escape directly through both the small cylinders to R. This gives a momentary time for the generation of the contrary motion in the piston, and the water in G G G, and greatly deadens the concussion which might else be produced.

Former attempts to make pressure engines, upon the principle of the steam engine, have failed, because water not being elastic could not be made to carry the piston onwards a little; so as completely to shut one set of valves and open another. In the present judicious construction the tumbler performs the office of the expansive force of steam at the end of the stroke.

There are several other engines of this construction at work. The general rate of working of all these engines is, that four feet

The Graphometer of Carangeau.



D.^r Thompson's Experiments

*To shew that Fluids are not
thrown into Currents
by Heat.*



Volta's Theory of Galvanism.



but the experiments offered to decide the question abound with difficulties.

Communication of heat through solids;

and fluids.

Circulation of fluids from heat.

The circulation does not *absolutely* prove the Count's opinion.

sion, that they conduct caloric in a very imperfect degree.

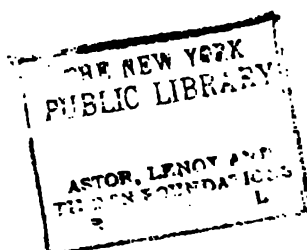
From a series of experiments which I have undertaken on this subject, I have been convinced, that investigations respecting it are liable to sources of error more important, and more difficult to be avoided than might *a priori* be supposed, and that we have no experiments sufficiently unobjectionable to decide the question. The observations I shall have to offer in the following memoir, will I trust justify these assertions.

When caloric is communicated to the surface of any solid substance, the temperature of the whole mass is increased with more or less celerity, and becomes uniform. In this case the caloric passes from one particle of the solid to another, and is thus equally distributed through the whole. When caloric is communicated to a mass of fluid, its temperature is likewise uniformly increased, and in this case also it was supposed, that the caloric is transmitted through the fluid, as in the former example through the solid. Count Rumford denies the possibility of such a transmission, and conceives the change of temperature in the fluid to be produced in another mode.

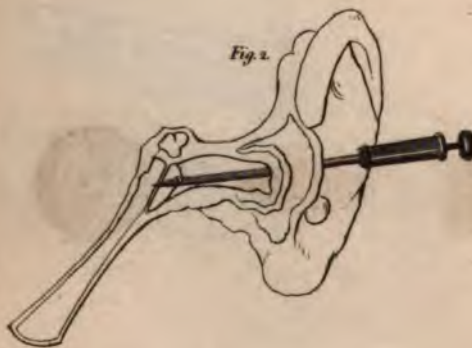
When any portion of a fluid is heated it must necessarily be expanded, its specific gravity must be diminished, and from its mobility it must change its place. If therefore caloric be communicated through a solid surface to a fluid, the portion of fluid immediately in contact with the solid, when heated, will acquire a specific gravity different from that of the mass; if the caloric is communicated from beneath, the heated portion of fluid must ascend, its place will be occupied by another portion, and thus the whole mass will be successively brought into contact with the heated surface, and will have its temperature increased. It is in this mode only according to Count Rumford, that the temperature of fluids can be changed. They can receive caloric from any solid matter with which they are in contact, but the heated portion communicates no part of its caloric to the rest of the mass, and the change of temperature which the whole suffers is owing solely to the motions of its parts.

That it is in a great measure owing to this cause, has been clearly demonstrated by Count Rumford's experiments. He has shewn, that when heat is applied to a fluid, motions of its parts take place, and that whatever retards or obstructs these motions, diminishes the celerity with which the temperature

is



Cure of Deafness by puncture of the Membr. tympani.



Manufacture of Gun-flints.



Fig. 1.



Fig. 2.



Fig. 3.



Fig. 4.



Fig. 5.



Fig. 6.



Fig. 7.



It cannot be known whether this be all that happens.

It may be supposed perhaps, that the quantity of caloric thus conveyed by the vessel would not be sufficient to produce the rise of temperature which took place in these experiments. But how is this to be ascertained. It may appear probable, but until it is proved, the point must remain doubtful, and the experiments must be therefore inconclusive.

To surround the vessel externally with water

It was an obvious idea, that this source of error might perhaps be obviated in a great measure by surrounding the vessel with water, which would carry off the caloric conveyed by its sides. It is evident, however, that it could carry off only part of it, for part must still continue to be abstracted from the internal surface by the water in contact with it. And even the quantity taken from the external surface of the jar by the water surrounding it, would be only somewhat greater than what would be carried off by the atmosphere when the experiment was performed as described above.

promised to be of some use. Experiment. Heat was found to descend in the internal and external water,

The experiment however of surrounding the vessel with water might afford some information: it would at least prove, that caloric was conveyed by the sides of the vessel, and it might indicate in some measure the quantity conveyed. I accordingly performed it in the manner represented in Fig. 2. D being merely a cylindrical glass vessel six inches in diameter, in which the apparatus used in the former experiments was placed. Water was poured into it, to the same height as in the internal vessel, and a thermometer was suspended in the water, the bulb being at precisely the same distance from the surface, as the bulb of the bent thermometer in the vessel A. The brass ball heated to 212° was suspended in the water in the latter vessel, and at the same distance from the thermometer as in the preceding experiments. The following were the results. At the commencement of the experiment, both thermometers were at 46° .

In 3 minutes, the internal thermometer

				A $46\frac{1}{2}$	the external E still 46
5 min.	-	-	-	$47\frac{1}{4}$	$46\frac{1}{2}$
10	-	-	-	$48\frac{1}{2}$	$47\frac{1}{4}$
15	-	-	-	$49\frac{1}{4}$	$48\frac{1}{2}$
20	-	-	-	$49\frac{1}{2}$	$48\frac{1}{2}$
30	-	-	-	49	48

Many reasons why this experi-

This experiment proves nothing more, than that a quantity of caloric is conveyed by the sides of the vessel. It might perhaps

perhaps be supposed, that by comparing the rise of temperature in the two thermometers, a conclusion might be drawn with respect to the mode in which the caloric was conveyed to them. If it were intirely conveyed by the sides of the vessel, since as much ought to be given out by the external as by the internal surface, the water without should be heated as much as that within, and consequently the two thermometers should rise equally, and to the same extent. But if the thermometer in the inner vessel rose more than that in the outer, it might be supposed, that the interposed fluid had directly conveyed to it part of the caloric from the heated ball above, with which it was more nearly in contact. But to establish such a conclusion from any differences of this kind, certain circumstances are indispensable, which are absolutely unattainable. It is requisite, for example, that both thermometers should be precisely on the same level, at the same distance from the sides of the vessel, and covered with the same quantity of water occupying the same volume. The smallest variation in these circumstances would produce a variation in the result. But if they were even obtained alike, the situation of the vessels must still remain very different; the one for example, presents a much more extensive surface to the surrounding medium than the other, and the nature of that medium is different, the internal vessel being surrounded with water, the temperature of which is augmenting while the experiment continues, the external with atmospheric air, whose temperature remains the same. It is impossible to estimate the differences in effect which must arise from these differences in situation, and consequently no conclusion can be drawn from the comparative alterations of temperature in the respective thermometers, as to the quantity of caloric conveyed to either of them by the sides of the vessel.

The preceding experiments then, or any of a similar nature, are incapable of determining the question respecting the conducting power of fluids. In all of them a quantity of caloric is conveyed by the sides of the vessel in which the experiment is made, and this quantity we have it not in our power to ascertain, so as to determine whether it is adequate or not to produce the augmentations of temperature which actually take place, and consequently whether any part of that augmentation is to be ascribed to a conducting power in the fluid.

The sides of the vessel will always conduct heat;

and no precaution can insure the object of the experiment.

Every precaution that can be taken to obviate this source of error, defeats the purpose of the experiment itself, since such precautions tend equally to lessen the effect which would result from a conducting power in the fluid supposing it to possess it, and it must therefore remain uncertain to what the diminished effect which takes place where they are employed, is to be attributed.

Other remarks.

If for example, the bulb of the thermometer be placed at a great distance from the matter communicating caloric, it will be a longer time before the thermometer begin to rise, and it will also not rise to so great a degree. But which ever opinion be adopted, whether that the fluid directly conducts caloric, or that caloric is communicated only by the vessel, this ought to be the case, since the circumstances necessary for its transmission in either way are rendered more unfavourable.

In like manner when a wider vessel is employed to contain the fluid, the rise of temperature must be less than when a narrow one is used, because in this case also there must be a larger portion of interposed fluid to be heated.

Or when a large portion of fluid is placed above the bulb of the thermometer, and in contact with the substance communicating the caloric, as with the ball in the preceding experiments, the same diminution of effect must take place, because the caloric given out by the ball being absorbed by a larger quantity of fluid, the temperature of that fluid must be less increased, and it must have less effect either in heating the sides of the vessel, or the thermometer.

General observation or result.

It appears therefore that in all experiments of this kind, a source of fallacy must be present: the effects of this will be more or less considerable according to the circumstances of the experiment, but they cannot be intirely obviated, nor their extent appreciated, so as to admit of a certain conclusion being drawn.

History of the experiments.

It may not be improper to remark, that the preceding experiments were made last winter, were stated in my course of lectures, and a concise account of them published in my Elements of Chemistry. I have now stated them partly to shew the insufficiency of experiments of this kind, either to establish or controvert Count Rumford's opinion, but principally as an introduction to those I have further to relate, and which appear to me to be free from the source of error I have stated.

This

This, of which I at one time despaired, may be attained by the simple contrivance of employing a vessel of ice. If a cylindrical vessel of ice be procured similar to that used in the preceding experiments, and if a thermometer be fixed in it, if on filling it with a fluid at the temperature of 32° , and suspending in it, at a short distance from the bulb of the thermometer, a heated solid, any rise of temperature take place, it may be considered as a certain proof of a conducting power in the fluid. No caloric could possibly be conveyed to it by means of the vessel, since ice cannot have its temperature above 32° , and there is no other mode by which the caloric can pass from the ~~ball~~ to the thermometer than by the interposed fluid. Of ~~the~~ experiments made to determine this point, an account will be given in a subsequent memoir.

III.

*Letter from Dr. VAN MARUM to Mr. VOLTA, Professor at Pavia, containing Experiments on the Electric Pile, made by him and Professor PFAFF, in the Teylerian Laboratory at Haarlem, in November, 1801.**

THE weather and season of the year preventing these gentlemen from attempting to charge the whole of the great Haarlem battery, consisting of 100 jars, in general nearly a line thick, and containing five square feet and a half of coating each, with the galvanic pile, they took five-and-twenty of these jars, which they charged separately, a few at a time, and all together; and uniformly found the single jars or the batteries charged to the same degree of intensity as the pile. They had taken twenty-six jars, but one of them did not receive the charge well, which they ascribe to the too great thickness of the glass.

They next charged the battery of $137\frac{1}{2}$ square feet with a greater or less portion of the pile, by soldering a hook to every twentieth plate of zinc, to which the insulated metallic wire for producing the communication between the battery and the pile might be conveniently attached. Beginning at

* Abridged from the *Annales de Chimie*, No. 120, Vol. XL. p. 289. C.

taking its electricity at different heights.

the fortieth pair from the bottom, the first where the separation of Bennet's electrometer was distinctly perceptible, the battery was charged to the same intensity as the electrometer indicated when brought into contact with that part of the pile, giving here a separation of one line. The effect was precisely analogous when the battery was charged by similar instantaneous contact with the pile at the sixtieth, eightieth, or hundredth pair, and so on.

The zinc being at top, the electricity positive.

In this pile, the silver being the lowermost of the metals, the electricity was positive at top, and the electricity communicated to the interior surface of the battery was the same, the contact being made with the superior part of the pile.

The metals being inverted, the electricity charged.

The order of the metals was then inverted, and the experiments repeated at different heights of the pile as before, with similar results.

Shocks taken by copper conductors two inches in diameter from the battery thus changed.

They now proceeded to examine the shocks given by the battery charged at different heights of the pile; for which purpose they employed two conductors of copper, two inches in diameter, held in wet hands. Beginning with the battery charged at the twentieth pair, they very distinctly perceived the passage of the stream from the conductor into the hand, and from the hand into the conductor. One gentleman present, Mr. Vander Ende, felt it as far as the wrists. When the battery was charged by forty pairs, real shocks were felt at the wrists: when by sixty, the shocks were very perceptible at the elbows: and they gradually increased, till the shocks extended to the shoulders with considerable strength, when the battery was charged by the whole pile.

The shocks given by the pile itself much stronger.

These shocks, however, were not equal to those given by the pile itself, but only about half their strength; the shock from a hundred pairs appearing to be equal to that of the battery when charged by two hundred.

Comparative experiments made with an electrical machine.

Having continued these experiments till nothing more could probably be learned from them, the comparative effects of the electrical machine, consisting of a plate of glass thirty-one inches in diameter, were tried with the same battery.

Precautions used in conducting these experiments.

This experiment requiring particular precautions, that the contact of the conductor might not impart more electricity to the battery than was furnished by the action of the machine during the contact, Dr. Van Marum formed a communication between the conductor and the ground by touching it with

one

one of his fingers, and withdrew his finger the moment he brought the large wire into contact with the conductor. This manœuvre he practised repeatedly, till he had acquired a habit of doing it with precision, before he began his comparative experiments; so that he was well assured, that the contact of the conductor, by means of the wire, took no more of the fluid than was furnished by the machine at the instant of contact. A single contact would not give the battery a change, that the electrometer was capable of indicating; but by repeated experiments it was found, that six of these contacts of the conductor imparted to the battery the same intensity as one contact of the pile. The power of the machine being equal to about half what the great machine possessed in its former state, from 1785 to 1789; but the great machine having gained considerably, particularly for charging batteries, by the rubbers of the new construction applied in 1790, and by the use of Kienmayer's amalgama, so that its power was rendered the quintuple of what it was before; the power of the pile of 200 pairs is to that which the great Teylerian machine possesses at present as 3 to 5. The ratio could not be examined directly, because a fire cannot be made in the museum, and the ground on which it stands is very damp in winter: this, therefore, is deferred till spring.

The shocks given by the battery, charged to different degrees of intensity by contacts with the conductor of the electric machine, were now compared with those it gave with similar intensities obtained from the pile; and reiterated trials convinced the experimentalists, that there was no perceptible difference between the sensations or shocks in the two instances, provided the intensities were the same. Hence Dr. Van Marum concludes, as the effects of such a considerable battery, when charged by the galvanic pile, are precisely similar, in every respect, to its effects when charged by a powerful electrical machine, the identity of the fluid put in motion by the pile with that moved by the electrical machine is proved so decisively, that no one will question them in future. These experiments, he adds, joined with those of Professor Volta, render extremely disputable, or rather completely refute the existence of a peculiar fluid, in all the other experiments termed galvanic.

The pile of 200 pairs had six times the power of the machine with a plate 31 inches in diameter,

and three-fifths of that the great Haarlem machine.

The shocks given by the battery, charged by the machine, not at all different from those it gave when charged by the pile to the same height:

the two fluids therefore presumed to be the same.

These

Extreme velocity
of the fluid.

These experiments have farther proved in a decisive manner, and on a large scale, another important circumstance respecting the pile; namely, that the stream of fluid moved by it has a velocity surpassing all conception; since a battery of $137\frac{1}{2}$ square feet was charged to the intensity it displayed by a contact as short as possible with the wire of communication, a contact that did not continue one-twentieth of a second.

Hence the effects
of the pile superior
to those of
common electrical
machines;

After this it is no way astonishing, that the pile, by a continuance of its action, should produce such effects as have never been seen where common electrical machines are employed, as the speedy decomposition of water for instance. Certainly no other electrical machine known, the grand Teylerian machine excepted, can furnish a continued stream at all approaching that of the pile; on which account it is a powerful mean of producing several effects, which may contribute greatly to the progress of natural philosophy.

and may be of
great use in physics.

Method of insulating
the pile.

This consideration induced Dr. Van Marum to endeavour to augment the power of the pile. From the beginning he was careful to insulate it more effectually than is commonly done. For this purpose he placed it on a thick cake of lac, and kept it in its vertical position by sticks of sealing-wax two inches long, fixed horizontally in four slips of wood, surrounding the pile. These sticks of sealing-wax were fixed to little wooden cylinders, passing through holes in the uprights at every four inches, and kept in their places only by being made to fit with such a degree of tightness, as would allow them to be moved backward or forward, so as to suit piles of different diameters. The uprights were fixed at top and at bottom in a piece of wood a foot square. Professor Pfaff himself was astonished at the great effect of the first pile composed of three Guilder pieces, plates of zinc of the same diameter, being an inch and a half, and bits of cloth moistened

Wire fused by it.

with a saturated solution of muriate of ammoniac. In one of the experiments the end of an iron wire, No. 16, $\frac{2}{240}$ of an inch in diameter, was made red-hot for the length of a line, and even fused at the extremity, by a pile of sixty pairs.

Having read an account of the experiments of Fourcroy, Vauquelin, &c. on the fusion of wire by large plates of copper and zinc, Dr. Van Marum procured thirty-two of each metal, five inches square, and made a pile with them; first

of

of ten inches in diameter, and eight pairs in height, by placing four of each metal together as one plate, they being exactly square, and their surfaces very smooth, that they might be in close contact; and then of five inches in diameter, and thirty-two pairs in height. The power of the higher pile greatly exceeded that of the larger. In August, Dr. Van Marum had fused completely into globules five inches of the iron wire mentioned above, and made seven inches red-hot, with this pile.

Pile of ten inches in diameter.

Another of five. The powers of piles depend more on their height, than on their diameter.

The number of pairs of metal being increased, the effect was not; fifty pairs producing less than twenty-five, by which nine inches of the same wire were melted when Professor Pfaff was present in November. This they ascribe to the moisture being too much pressed out of the pieces of pasteboard by the weight of so many plates of metal a line and a half or two lines thick; as the upper twenty-five pairs, being taken off, had as much effect as before, while the lower twenty-five had not half the power. In consequence they divided the pile into four, containing together 110 pairs. The plate of copper, placed under and connecting the two piles that were insulated, had a rim, that the solution pressed out of the pasteboard might remain in it, and not wet the insulating cake, and destroy the insulation. These two piles, containing sixty pairs somewhat thinner than the others, when not connected with the other two, made six inches of the wire red-hot. The other two piles, containing fifty pairs, made eight inches very red, and fused great part of this length. This difference was ascribed to the pasteboards not being sufficiently moistened in the former instance. The four piles connected together made twelve inches of the wire red-hot.

Effects not increased in proportion to the height, when the plates are large and thick, because too much fluid is pressed out.

On this account the pile should be divided into several, communicating with each other.

On taking sparks repeatedly at each extremity of the connected piles, by means of a wire communicating with a basin containing quicksilver, no difference between those of the positive and those of the negative electricity could be discerned. Whether issuing, or entering, they appeared radiating, when iron wire was employed; which the experimentalists ascribe to the combustion of the iron, for when a wire of platina was used, no rays were perceptible in the sparks of either electricity. Sometimes the communication was made by means of a needle, fastened to the slender wire

Positive and negative sparks did not differ.

Beautiful phenomena produced by the combustion of the wire.

which was fixed to the end of the conducting wire; at other times by the less pointed end of the conducting wire itself.

When the surface of the quicksilver was touched with the end of a slender wire instead of the needle, a very striking phenomenon ensued: the combustion of the extremity of this wire was effected with such force, that it threw out sparks on all sides, which formed thousands of apparent rays, representing beautiful suns some inches in diameter. By gently lowering the end of the wire in proportion as it was dispersed by the combustion, this appearance could be continued at will. It is seen in some degree likewise when the quicksilver is touched with the point of a fine needle; but then it is of short duration, as it ceases as soon as the needle has left its point. The experiment was repeated with wires of $\frac{1}{32}$, $\frac{1}{16}$, and $\frac{1}{8}$ of an inch in diameter: that of the middle size gave the largest and most brilliant suns, but the smaller succeeds better when the power of the pile is less.

Quicksilver oxidized on its surface by the sparks.

On taking wires too thick to be melted, the oxidation of the quicksilver at its surface by each spark was more distinctly seen, spots of upward of a line in diameter being formed.

Platina fused by the pile.

The extremity of a wire of platina, about $\frac{1}{16}$ of an inch in diameter, was melted, and formed a globule.

Sparks upward of a line in diameter.

The sparks issuing from the end of the communicating wire, when it was not too slender, were more than one-tenth of an inch in diameter.

Piles of an equal number of pairs, though different diameters, give equal intensities, and similar shocks; notwithstanding they differ so much in fusing metals.

On comparing the intensity of the electricity of this pile with that of the former of zinc and silver an inch and a half in diameter, both by the simple contact of the Doctor's most sensible electrometer, and by the condenser and an electrometer of less sensibility, they were found precisely the same, when the same number of pairs were used: they charged the battery before described to the same height: the shocks given by the battery thus charged were in both cases precisely the same: and in those from the piles the difference was scarcely perceptible.

Attempt to account for this.

It appears strange, that two piles so equal as to intensity, and charging large batteries in such an equal degree, should produce such different effects in fusing metals. To form a just notion of this, observes Dr. Van Marum, we must distinguish the action of a pile which is insulated from that of one which is not. In the latter there is a stream passing continually from

from one extremity to the other, and through the conductor or chain of conductors which connects the two extremities: but this stream cannot take place in an insulated pile. From this single distinction it appears, that the equality of the intensities of insulated piles affords no reason to expect an equality of the streams of the two piles when not insulated: on the contrary, if the stream meet with less impediment in a large pile, than in one of smaller diameter, the stream will produce a greater effect, in consequence of its greater velocity.

To see how far this velocity depended on the size of the pieces of pasteboard employed, two piles were constructed, each of twelve pairs of the large plates, in one of which the pasteboard was cut to the size of the plates, in the other to the diameter of half an inch only. The intensities of both were the same, yet the other effects were much greater in the pile with large pasteboards; for it gave very brilliant sparks, which began with the fifth pair from the bottom, while the other pile scarcely emitted any that were perceptible. On making the pasteboards somewhat larger than the metallic plates, the effect was by no means increased, but rather diminished. It was found necessary, likewise, that the pasteboards in the large pile should be well wetted.

Effects of different sized pasteboards.

These should not be too large: and should be well wetted.

There is another apparent anomaly, however, in the effects of piles differing in diameter: though the action of the large pile is so much more powerful in fusing metals, not the least difference can be perceived in the shocks they give: and this Dr. Van Marum confesses is not accounted for by his hypothesis.

Anomaly not accounted for by the hypothesis.

Some farther experiments were made respecting the efficacy of the solution of muriate of ammonia, which was found to be preferable to common water, or to a solution of muriate of soda, for moistening the cloth or pasteboard. A pile of twenty pairs with the ammoniac solution fused into globules four inches of wire, No. 16; while that with solution of common salt did not fuse or even redden a single line, and that with simple water shewed still less effect with regard to the sparks it produced.

Solution of muriate of ammonia preferable for moistening the cloths.

As several authors have ascribed the greater effect produced by saline solutions to the oxidation of the metals, many experiments were tried, with a view to ascertain this point,

Not because it oxidates the metals, for it is said to be super-

rior to the more powerful acids. by moistening the pasteboards with sulphuric, nitric, and muriatic acids, both in a concentrated state, and more or less diluted. The particulars are not given, but the results are said not to have favoured the supposition, on a comparison of the intensities, shocks, or sparks; all of which were stronger when muriate of ammonia was employed, than when nitric acid was used, either concentrated or diluted, by which the two metals was very speedily oxidated. As nothing is said here on the fusion of wire, this does not appear to have been tried.

Ammonia alone not equal to the muriate. Effects of the pile in the air, in vacuo, in carbonated hydrogen gas, and in azote gas, equal:

but in pure oxygen gas much increased.

A pile with potash was very strong, but showed no oxidation.

Very large pile of five inches square.

Ammonia alone being used to moisten the pasteboards, the effects produced were all less than with its muriate.

Experiments were likewise made in vacuo, and in different airs. The apparatus employed very readily produces a vacuum in which the barometer falls to below one line; but in this instance the vapor produced by the water of the pile kept the quicksilver at the height of five lines. Between the effects produced in the open air, in this vacuum, in carbonated hydrogen gas, and in azote gas, no difference could be discovered. On the introduction of pure oxygen gas, however, the sparks were much larger, more brilliant, and easier to be obtained: but on making a vacuum after this, the shocks were feeble, and the sparks smaller, than they had been in any of the former instances. Oxygen gas being introduced a second time, its effects were as powerful as before: the cylinder being emptied again, the effects of the pile were reduced as much as before: and on letting in atmospheric air they were restored to the same degree nearly as in the former experiments.

An experiment of the Doctor was directed to ascertain the presence of oxidation in the use of a very concentrated solution of potash in a pile of twelve pairs of five inches square, which produced much more effect than another similar pile with water only. But on separating the pile neither the copper nor the zinc had suffered any injury in their polish. The Doctor does not say how long the pile stood together.

Lastly, The galvanic apparatus was increased to 200 pairs of zinc and copper in six connected piles of five inches square each. With this he fused an iron wire, No. 16, of twenty-three inches entirely into globules, and ignited the whole of another piece of thirty-three inches.

The

The shock was tried of a column of twenty pairs of copper and zinc of one inch and a half, and of another of the same number of plates of five inches; and also of another of ten inches square; but not the least difference could be perceived. The last mentioned pile (which, as well as the others, was moistened with muriate of ammonia), fused five inches of wire; and the other of five inches square fused four inches. Whence it seems to follow that the effects of these piles, as to the fusion of wire, do not increase in the proportion of their surfaces, but in a less ratio.

The shocks not at all increased by the increase of surface.
Fusion of wire does not increase as the surface, but in a less ratio.

IV.

*A Reply to Mr. CRUIKSHANK'S Observations in Defence of the New System of Chemistry, in the Philosophical Journal *. By JOSEPH PRIESTLEY, L. L. D. F. R. S. &c. Communicated by the Author.*

HAVING proposed to philosophers the re-consideration of the doctrine of *phlogiston*, which for some time has been almost universally exploded, I am happy to find so truly ingenious and candid a person as Mr. Cruikshank has given some attention to the subject. That experiment of mine which he particularly examines, is that in which I procured a very large quantity of heavy inflammable air from finery cinder and charcoal, both previously exposed to such a degree of heat, as would have expelled from them all the air that mere heat could expel. This I ascribed to the water in the finery cinder uniting with phlogiston from the charcoal. Mr. Berthollet thinks, that this inflammable air comes from the decomposition of the water contained in the charcoal, and Dr. Woodhouse, from that which he allows to be retained in the finery cinder. But Mr. Cruikshank, not satisfied, I presume, with either of these hypotheses, has suggested a very different one, for he asserts, that water is not at all necessary to the production of this inflammable air, maintaining that metals, and their calces, in a very high temperature, have the power of decomposing fixed air, and in this case the fixed air must be formed from the oxygen in the finery cinder, and the carbon in the charcoal.

Short preface and recapitulation.
The heavy inflammable air from finery cinder and charcoal, ascribed to water either simple or decomposed;
but by Mr. Cruikshank to a decomposition of carbonic acid.

His experiments with metallic oxides and charcoal, in proof,

are inferred by Dr. P. to prove that the oxides contain water.

Iron or its oxide heated by the solar focus in carbonic acid is stated to have produced no decomposition;

for though part was rendered immiscible with water by thus heating either an oxide or a piece of earthen crucible in it;

yet there was no addition of oxygen, but only azote.

Mr. C. produced heavy inflammable gas from iron filings and chalk heated;

After repeating my experiment, which he found to be just, Mr. Cruikshank did the same with the calces of other metals, particularly those of zinc, copper, lead, and manganese, and then concludes, p. 4, that in all these cases "the air must come from the partial decomposition of the carbonic acid by the calx when raised to a high temperature." But the inference that I think is more naturally drawn from them is, that all these calces contain much water, and little or nothing else. This I have shewn to be the case with respect to several of them, especially that of zinc; though I doubt not but that some small portion of oxygen may be contained in them all. Indeed, we cannot absolutely say, that any substance whatever can be wholly expelled from any other, with which it has been chemically combined by any process.

Before Mr. Cruikshank admitted that iron, or its calx, when raised to a high temperature, can decompose carbonic acid (*i. e.* fixed air) in this experiment, he should have tried whether it would do it in any other. If in any case, I should think it would do it when it was heated in this air by a burning lens, by which a greater heat may be produced than in any open fire. But this I found not to be the case either with iron, or this calx of it. In the last summer I went through a course of experiments with this view; but I always found fixed air not to be decomposed by this means. Though I found that a portion of this air, and also of all the other kinds that are readily imbibed by water, was rendered immiscible in water by means of heat reflected either from a calx of any metal, a piece of earthen crucible, or any other substance, on which I threw the focus of the lens when it was surrounded by this kind of air confined by mercury or water: This, however, was no decomposition of the air, as there was no oxygen found in it after the process. The addition of permanent air was always phlogisticated.

Mr. Cruikshank thought, that if this heavy inflammable air came from the decomposition of the carbonic acid by the iron, he should succeed better, p. 4. by employing iron filings in the place of finery cinder, as they would have a greater affinity with oxygen; and with this view he heated them together a quantity of common chalk, previously exposed to a low heat, for ten minutes. From this mixture he procured a great quantity of air, and he thought that the acid (*i. e.* fixed

air

air from the chalk) was decomposed by the iron; whereas when he used well burned lime he got little or no air. What I infer from this experiment is, that the chalk, not being perfectly calcined, contained some water, as well as fixed air, and that this water uniting with the phlogiston of the iron formed the inflammable air that he found. Water I suppose to be the basis of all the kinds of air, and many substances retain it in any degree of heat. Chalk I have found to do it after long exposure to the heat of a smith's forge.

Admitting the fixed air procured in the experiment with the finery cinder and charcoal to come in part from the oxygen in the finery cinder, how is this oxygen to be expelled from the calx, since heat will not do it? And there is no instance, I believe, in chemistry, in which when heat alone will not expel any constituent part of a substance, it can be effected without the aid of an affinity, in consequence of which some other substance takes its place. But here, according to the new theory, nothing is supposed to take the place of the oxygen in the finery cinder. It takes nothing from the charcoal, but the iron is revived by the mere expulsion of the oxygen.

Mr. Cruikshank lays great stress on the difference that he found in the air that he procured in these processes from that which is got from charcoal and water. But I have observed, that there is a considerable difference in the qualities of heavy inflammable air, not only according to the substance from which it is procured, but in the successive stages of the same process. He will find that I have examined this kind of air as procured from a great variety of substances, made to pass in the form of vapour through hot earthen tubes, and in various other ways, and have given the analysis of them. I always found that the first portions from charcoal were loaded with fixed air, but that in the course of the process this disappeared, the air burning with a lambent flame, and that towards the end it approached to the explosive kinds, as obtained from metals by acids.

I also found that more or less fixed air is procured by the decomposition of heavy inflammable air by means of dephlogisticated air; and though the air procured from finery cinder and charcoal shewed no sign of its containing any mixture of fixed air, nothing of the kind being discoverable by lime

which the Dr. does not infer to be carbonic acid decomposed; but water with phlogiston from the iron.

If the carbonic acid, from finery cinder and charcoal, derive its oxygen from the oxide, this principle must, it is stated, be transferred contrary to the laws of affinity.

The difference between this gas and that from mere water and charcoal, stated to afford no conclusive argument.

Oxygen combined with heavy inflammable air affords carbonic acid, in greater quantity than the latter; which

water;

is inferred to
have afforded
phlogiston.

water; yet when it was decomposed I found much more than the weight of the air; so that it could not have been previously contained in it in a state of solution, but must have been formed by the union of the oxygen in the dephlogisticated air, and the phlogiston in the inflammable air.

Charcoal sup-
posed to contain
carbon and
phlogiston.

That charcoal uniting with water should give fixed as well as inflammable air, I account for by supposing, what is by no means improbable, that this substance contains the elements of both the kinds of air, and that they want nothing but water to enable them to take the form of air.

Conclusion.

I hope that Mr. Cruikshank, with the same candour with which he has began this discussion, will re-consider his hypothesis, and extend his examination to my other arguments in defence of the doctrine of phlogiston, and against the decomposition of water. Nothing but free discussion is necessary to the discovery of truth, and it is desirable that error should be detected as early as may be, especially if the consequence of its reception be extensive and important.

Your's sincerely,

J. PRIESTLEY.

V.

A Statement of the Experiments made by the Rev. ABRAHAM BENNET, F. R. S. on the Electricity produced by the contact of Metals previous to the Year 1789, and also of those made by Mr. TIBERIUS CAVALLLO, F. R. S. previous to the Year 1795, to which Allusion was made at Page 114 of this Journal.
W. N.

Short statement
of Bennet's ex-
periments, and
why they are
here repeated.

SOMETIME previous to the period first mentioned in the title to the present sketch, Mr. Bennet had remarkably increased our power of measuring small intensities of single electricity by the application of gold leaf to the bottle electrometer of Cavallo, and by the process known by the name of doubling; an outline of the history of which may be seen at page 396 of the first volume of the Philosophical Journal, in quarto. His *New Experiments on Electricity*, which is a thin quarto of 141 pages, were published in 1789 by subscription; and may perhaps have been less universally diffused in the
scientific

scientific world, than if the book had appeared in the usual mode of publication. At all events, as the work is now scarce, and the subject of some interest so far as it may be thought connected with the theory of galvanism, I think it will not be unacceptable to my readers to relate what he did at that early period. The same reasons will in part apply in favour of a concise account of the experiments of Mr. Cavallo.

Soon after the publication of Bennet's doubler in the Philosophical Transactions for 1787, he found that the instrument produced electricity without previous communication, and that it always retained that property, whatever care might be taken to deprive it of any adhering charge. When he was afterwards engaged in a course of experiments facilitated by the mechanism I applied to the doubler in 1788, he found that a very great portion of this adhering electricity might be removed, by turning the handle of the doubler a considerable number of times, while all the plates were connected with the earth, and that by virtue of this provision the instrument might be trusted to indicate the nature of communicated electricity, to a degree of accuracy far exceeding that which could be afforded by any simpler instrument.

Uncertainty of the electrical doubler; from spontaneous electricity,

removed by working its parts uninsulated.

By reasoning upon this phenomenon, he was induced to conjecture, that this spontaneous electricity was not owing to accidental friction, but to what he called the increased capacity of approximating parallel plates, which might attract and retain a charge, though neither of them were insulated. The experiments he made in support or proof of this hypothesis were the following:

Capacity of bodies for electricity supposed to increase by approximation.

He repeatedly tried the effect of depriving the doubler of spontaneous electricity, by turning the revolving plate forty times, with brass wires hooked to all the plates, and he found that if he took off the wires while the revolving plate stood at a distance from the two stationary plates, it was more completely deprived of electricity, than if the wires were taken off when the revolving plate stood parallel to the plate A. That is to say, it required 21 revolutions to exhibit spontaneous electricity after the provision in the first case, and 16 revolutions in the second case. Whence it seemed to him, that the two plates standing parallel to each other, had by an increase of capacity acquired a certain small charge, which

Experiments. The doubler sooner exhibited electricity when cleared and left with its plates opposite, than when at a distance from each other.

sooner

sooner rose by doubling to a perceptible quantity, than that electricity which they might have possessed in the first case.

An insulated copper plate placed in contact with a table, with a large approximating surface became negative.

But to make the experiment in a more direct manner, using the doubler only as an instrument of admeasurement, he took a copper plate thirteen inches in diameter, having its surface rather convex, furnished with an insulating handle of oiled glass four inches long, and baked till the whole was well hardened. One end of this glass was fixed into a copper socket in the middle of the plate, and the other end into wood, that it might not be necessary to touch the electric part of the handle. This copper plate was placed flat upon a mahogany table, and the doubler being deprived of its electricity, the revolving plate B was placed parallel to A, so that B was connected with the earth; then the copper plate was lifted up by its insulating handle, and applied to the plate A. And lastly, the plate B being revolved only five times, caused the gold leaf of the electrometer to diverge negatively to the distance of a quarter of an inch.

The same result when a surface of water was used instead of the table.

It might in this instance easily have been stated, that the contact of the copper plate with the mahogany table, did in fact produce electricity by excitation or friction. But to obviate this, he first repeated the experiment, by touching the copper plate with the point of a needle, and then applying it to the doubler as before, and he found that the instrument did not produce its spontaneous electricity in less than 15 revolutions; after which touching the copper plate again with the needle, he applied it to the surface of some water contained in a large dish, so that its convex part touched the fluid; and then lifting it up he applied it to the doubler, and electricity was communicated, sufficient to cause the gold leaf to diverge negatively at five revolutions, as in the former experiment.

When the touch was reversed the opposite power was obtained.

The last experiment with the water was repeated, with this difference only, that the application of the copper plate was made to the revolving plate of the doubler, instead of the plate A, the revolving plate being moved a little past its contact with the ball, and the plate A being at that time made to communicate with the earth by the brass wire; five revolutions in this case produced electricity as before, but the electricity of the plate A was now positive, as might be expected in consequence of the negative charge having been communicated to B instead of A.

The

The foregoing experiments were frequently repeated, but as the charge had hitherto been negative, he was desirous of knowing, whether a variation of the nature of the surface might not alter it. He therefore covered the surface of the copper plate with a mixture of gum water and minium, and also with gum water and wheat flour, and he found that this substance when dried upon the surface of the copper, changed the nature of the charge communicated to the plate to which it was applied.

When the plate was covered with gum water, &c. and dried, its electricity became positive.

Lastly, to render what he called the electricity of the approximating plates more conspicuously sensible, he ground a brass plate three inches diameter with emery, till it would adhere to the surface of a piece of black marble. This plate and marble therefore constituted a condenser in its original state. The marble being moderately warmed, he pressed the brass plate upon its surface with the point of a brass wire; then lifting it up with its insulating handle he applied it to the cap of the electrometer, which caused the gold leaf to strike the side negatively.

The condenser affords electricity.

Hence he thinks it must appear evident, from the precautions and experiments he has stated, and from the known laws of electricity. 1. That the doubler may be deprived of accidental or communicated electricity. 2. That the principal cause of its spontaneous charge, is the attraction of electricity by the approximation of its parallel plates. 3. That this charge may be positive or negative, according as the plates or touching wires are composed of substances which have a greater or less adhesive affinity with the electric fluid. 4. That the causes of spontaneous electricity are common to the condenser, both in its original and improved state, and the doubler, and equal in them all, as far as they are equal in their dimensions and powers. 5. That since the doubler may be composed of very small plates, and yet its power be equal to that of a very large condenser, its spontaneous electricity will be more easily overcome by a communicated charge than that of a condenser of equal power, and therefore experiments performed with it will be less liable to equivocal results.

Enumeration of results. 1. The doubler cleared of electricity. 2. Cause of spontaneous charge. 3. and its nature. 4. Common to all condensers. 5. The doubler preferable.

This author proceeded to make other experiments, on the so termed adhesive electricity of metals, and other conducting substances. He deprived the doubler of its spontaneous charge, and placed the two plates A and B opposite

Other experiments of adhesive electricity; with iron and steel;

each

each other, but so that B was not connected with the earth. He then touched the plate A with the blade of a knife, and the plate B at the same time with the point of a softened iron wire. With sixteen revolutions the gold leaf diverged about one third of an inch positively. The doubler was then again deprived of electricity, and the revolving plate B placed as in the last experiment. The knife was applied to B instead of A, and the soft iron wire to A instead of B, which opened the gold leaf negatively at fifteen revolutions. These experiments were repeated very often, and the electricity changed each time; being always positive in the plate touched with the knife. He remarks, that it would appear incredible, that so minute a difference of adhesive electricity as that which might be supposed between two metals so nearly alike as hardened steel and soft iron could be distinguished, had not the frequent repetition of experiments confirmed it.

with other substances,

He proceeded to make similar experiments, which are tabulated, of different substances, namely, lead ore and lead; lead and iron wire; lead ore and iron wire; tin foil and iron wire; zinc and iron wire. All the experiments having been made by double contact and alternate application of the opposite substances to the plates A and B, he also by single contact determined whether the adhesive electricity, or to state the fact more unexceptionably, the electricity produced by each substance in the doubler, was positive or negative. I have thought it less immediately necessary to state the particulars of these experiments, because I presume that the substances were held in the hand, and from a variety of facts, I think we are justified in concluding that friction, or a contact equivalent to friction, between the human skin and the substance thus held, will produce electricity more than sufficient to render it unnecessary to look for any other cause, or at least quite sufficient to render our investigations uncertain when so conducted. That is to say, I think the results will rather consist of determinations of the kind of electricity produced by friction of the hand upon the several substances, than any new or peculiar affection of the substances themselves.

Objection from the effect of friction by the hand.

Theory of electric excitation.

Among other interesting observations, for which I must refer the reader to the work itself, he gives a simple theory of the excitation of glass, and other electrics used in the construction of our machines. He remarks, that when the silk

flap

flap is rubbed by the revolving glass cylinder it is brought into close contact; and the electricity adhering more forcibly to the glass, is carried forward into the open air, which air not having been rendered negative like the silk, does not counterbalance the surface of the glass, and therefore its capacity being lessened, it emits the charge it had just absorbed. And he thinks the amalgamated cushion assists the process, by bringing a surface of a conducting quality, and in connection with the earth into closer contact.

The experiments of Mr. Cavallo were made by letting substances fall out of his hand for the most part upon an insulated plate of tin, whence they were shook off upon a table, or upon a chair; from which he again took the substance up, and let it fall upon the tin plate to the number of times required. After these reiterations he applied his tin plate to the plate of his multiplier. In his experiments a piece of zinc little more than half an ounce, was dropped ten times successively upon the plate, which last, as appeared by the subsequent operations of the multiplier, was electrified negatively. Another piece dropped ten times produced the same effect. Zinc was heated to 110 degrees, and one repetition of the experiment produced the same effect, but stronger. A shilling, a half crown, a new guinea, a piece of copper, a piece of malleable platina, with like manipulation, produced similar effects, but differing in degree. Platina produced very little electricity; but when heated it was found to produce an effect contrary to the preceding; that is to say, it electrified the tin plate positively. A piece of lead appeared to produce negative electricity in the tin; but when hot positive. A piece of iron afforded very equivocal results. A piece of grain tin afforded negative electricity, as well when heated as when cold.—When the cold tin was dropped from a pair of iron tongs, and let fall from the tin plate upon a chair, whence it was picked up by means of the same iron tongs, it produced weak positive electricity. Tin when heated afforded electricity of the same quality, but greater in quantity. Mr. Cavallo alternately used the hand and the iron tongs with results generally like the preceding, that is to say, negative electricity in the tin plate, when the grain tin was dropped from the hand, and positive when it fell from the tongs. Bismuth produced positive electricity; but when the bismuth was made very hot,

Cavallo's experiments of electricity afforded by the fall of conducting bodies upon a tin plate.

only electricity
was produced
when the tin
was heated

the

Cavallo's experiments of electricity afforded by the fall of conducting bodies upon a tin plate.

the electricity of the tin plate was negative. These results were the same, when the bismuth was cast into a smooth flat piece, instead of being broken from a lump. When the iron tongs were used with the bismuth, the tin plate became negative, contrary to what it was when the hand was used.

An insulated silver spoon was substituted in the place of the tin plate, and in this zinc dropped from the hand produced negative electricity, which was stronger when the zinc was heated. The kind of electricity was not changed when the spoon was heated, and the zinc cold, but its degree was much less. Mr. Cavallo considers it as a very extraordinary fact, that the experiments varied as to the intensity of the electricity very much on different days, and is inclined to ascribe this difference to the disposition of the atmosphere.

In order to discover the source of the electricity produced in these experiments, he repeated them in a great variety of ways, namely, instead of the hand, he dropped the zinc from a tin plate held with one hand into a spoon, and from the latter back upon the former. He performed this operation with both the tin plate and the silver spoon insulated. He likewise tied a silk thread to the zinc, and holding the other extremity of the thread in one hand, struck the zinc repeatedly against the spoon; but in those cases very seldom any electricity was manifested, except when the weather, and every other circumstance was very favourable, and then the electricity could with difficulty be manifested; yet when the tin plate was held in the hand, and the zinc thrown from it into an insulated spoon, some electricity was more frequently produced than in the other two cases.

Whether animal electricity can be deduced from the preceding facts? &c.

After a careful review of his experiments, he doubts whether the phenomena of animal electricity can be attributed to the cause supposed to operate in them. For as he remarks, the action of metallic bodies produces the same effects, with hardly any observable difference, upon prepared animal limbs; whereas the effects in his experiments were fluctuating, and differed considerably in zinc and bismuth, which nevertheless do not excite the animal electricity more powerfully than zinc and silver, or zinc and gold. He also remarks, that he found by experiments with minute quantities of electricity applied to the prepared limbs, that they were not excited when these quantities were nevertheless very much greater than what was produced

produced in his other experiments. But on the whole, he thinks they seem to establish, 1. That the contact of one metallic substance with another generally produces electricity. 2. That the quantity and quality of the electricity so produced, is various, according to many circumstances which seem to occur in the products of it, or in a great measure to influence it. 3. And that these circumstances are, the various nature of the metallic substances, their various degrees of heat, the state of the atmosphere, the hand of the operator, &c. each of which causes has its share in the result.

VI.

*On the Formation of Crystals, describing a Method of producing them large and regular. By CIT. LE BLANC *.*

IT had long since been remarked, that the same salt is susceptible of crystallizing under several different forms. Haüy has demonstrated, that all these secondary forms are owing to different arrangements of the same integrant molecule; he has shewn that these effects are not the effect of what is termed chance, but that they proceed from laws sufficiently simple, which may easily be determined. Here he has stopped; he has not yet thought proper to publish the ideas which he has given in his course [of lectures] relative to the causes which dispose the integrant molecules to follow such or such a law in their mutual arrangement. These are the causes which Cit. Le Blanc investigates in his observations on the growth of crystals. He has been long engaged in these inquiries, and the memoir which he has read to the Institute is a confirmation and sequel of that which he read to the Academy of Sciences, of which an extract was inserted in the *Journal de Physique*, November 1788, p. 374. He has discovered, by his persevering and ingenious observations, that we may considerably vary both the bulk and form of the crystals at pleasure, by causing them to be formed and to grow under certain circumstances, and he has long since en-

On the causes of crystallization.

Crystals of extraordinary size and beauty.

* Communicated to the French National Institute, and abridged in the *Bulletin des Sciences*, whence the above is translated, No. 50, An. 10.

riched the collections with crystals of alum, sea-salt, sulphate of copper, &c. of an extraordinary size and beauty: he now publishes the means which he has employed.

How made.

Flat-bottomed vessels of glass or porcelain, are the best for obtaining beautiful single crystals. The solutions ought to be brought to the point of crystallization. They first yield crystals that are very small. Amongst these small crystals, which

**Embryo crystals
nursed or reared,**

Cit. Le Blanc calls embryos, a selection is made of the neatest, in order to promote their growth, or as C. Le Blanc terms it, to *nurse* (*elever*) them. The liquor is decanted in order to purify it, and the small crystals that have been selected are distributed in it, and carefully turned every day. Amongst these crystals a second selection is made, in order separately to nurse those of which we wish either to augment the volume, or change the form.

**by selecting the
placing them in
the mother wa-
ter.**

In order to make them grow without irregularity, they must be placed in the mother-water of a solution that has afforded a confused crystallization. Care must be taken to turn them often, and to give them fresh supplies of mother-water in proportion as they increase in growth. In this manner they may be brought to a considerable volume.

**They may be
made to grow
either in length
or breadth.**

If they be left too long in a solution in which they have acquired their full growth, they diminish instead of increasing in size, and it is observed that this decrease takes place at the angles and edges, so as to leave striæ visible, which indicate the direction of the ranges of subjacent molecules.—The position of the crystals in the solution influences their form: this is particularly remarkable in the prismatic crystals: they grow in length when they are laid upon one of their sides, and in breadth when they are placed upon their base.

Secondary forms.

Cit. Le Blanc having changed octahedral alum into cubic alum, by placing an octahedral crystal in a solution of alum saturated with its earth, which gives the cube, infers from thence that frequently the secondary forms are owing to differences in the proportion of the principles*.

A curious

* It appears to us that this fact cannot lead, more than any other, to such an inference. According to the experiments of Cit. Vauquelin, the alumine in excess is mixed with the sulphate of alumine, but it is not combined with it; for simple solution in water is sufficient

A curious observation of Cit. Le Blanc, which has already been recorded in the *Journal de Physique*, proves that the same solution left to itself, is not equally saturated in all its parts. If we suspend crystals at different heights in a solution, the lowest crystals increase more rapidly than the higher ones; and it sometimes happens that these dissolve whilst the lower still continue to grow. Citizen Le Blanc adverts to the analogy that subsists between this observation, and that of the more complete saturation of the water of the sea at great depths.

Cit. Le Blanc informs us, that by adding sulphate of copper which crystallizes in oblique prisms, to sulphate of copper which crystallizes in the octahedral form, rhomboids are constantly obtained *.

Solutions are more saturated at greater depths.

VII.

Accounts of the New Planet CERES †.

ON the 4th of February a letter from Dr. Maskelyne was read before the Royal Society, announcing that he had observed the new planet of Mr. Piazzi passing the meridian between three and four o'clock in the morning, having about $138^{\circ} 43'$ right ascension, and $12^{\circ} 38'$ north declination, appearing like a star of the eighth magnitude.

Letter to the Royal Society, announcing observations of the new planet by Dr. Maskelyne, Mr. von Zach, and A. Aubert, Esq.

Another letter from Mr. von Zach was read, informing the Society that he had observed this planet at Sceberg on the 7th of December, within half a degree of the place before

cient to separate it. These crystals are accordingly opaque: besides, the same chemist has obtained cubic and transparent crystals from acidulous sulphate of alumine. (Note of the Editors).

* We must observe that the primitive form of the sulphate of iron is the rhomboid, and that the irregular octahedron which it presents is a secondary form. Cit. Haüy has examined one of these crystals resulting from a mixture of a solution of sulphate of copper with a solution of sulphate of iron. The rhomboid which he examined differed in no respect from the primitive rhomboid of the sulphate of iron. (Note of the Editors.)

† This whole article is taken verbatim from the *Journal of the Royal Institution*.

determined in his journal. Mr. Olbers saw it at Bremen on the 2nd of January. With a power of above 120 it presented no observable disc.

On the 11th a second letter from the Astronomer Royal informed the Society that he had repeated his observation of the new planet, so as fully to ascertain its motion. It appeared to have a visible disc when on the meridian, and viewed with a power of 50. When the air was very clear the disc was round and well defined, but somewhat smaller than that of the 34th of Virgo, a star of the 6th magnitude near it. Dr. Maskelyne observes, that the smallness and roundness of the appearance of the disc of the fixed stars is a good criterion of the clearness of the air.

Another letter from Alexander Aubert, Esq. F. R. S. was also read. Mr. Aubert discovered the planet Ceres on Sunday morning, having about $188^{\circ} 41'$ right ascension, and 13° declination, its motion at present being retrograde.

History of the planet as discovered by Piazzi, observed by Olbers, and considered by Oriani, Zach, Bode, Lalande, and Burckhardt.

Citizen Burckhardt in the Moniteur, 4 Pluv. An. 10. No. 124. gives the following account. The planet which Mr. Piazzi discovered at Palermo the first of January 1801, was again seen the first of January 1802, by Mr. Olbers, at Bremen, nearly in the place where it was expected from the calculations of Mr. von Zach. The 2nd January 1802, at $18^{\text{h}}. 58' 36''$, mean time, at Bremen, its right ascension was $185^{\circ} 9'$, and its declination $11^{\circ} 9'$ north, in the wing of Virgo, near a star of which Lalande had given the position, in the *Connaissance des Temps*, Year 9, p. 254. The 5th January, at $17^{\text{h}}. 36'$, its right ascension was $185^{\circ} 43'$, and its declination $11^{\circ} 8'$, nearly. It appears as a star of the ninth magnitude, but it will become more conspicuous. With a telescope magnifying 106 times, it cannot be distinguished from a small star.

The 1st January it fortunately made a right angled triangle with two small stars mentioned in Lalande's *Histoire Céleste*; the following day the form of the triangle was changed, and by means of this change the planet was recognised. It will be on the parallel of the 20th of Virgo.

The elements of this planet have occupied several astronomers. Messrs. Oriani, Zach, and Bode, had suspected at once that it was a planet, because it had been observed stationary, and without nebulosity. But having received only two complete observations, they had not been able to confirm their

their suspicions. Some time afterwards, Mr. Lalande first obtained a complete copy of the observations of Mr. Piazzi, who could not refuse them to one, under whom he had so long applied to the study of astronomy. By means of these observations, I was the first that demonstrated, in a memoir presented to the National Institute, that there was no parabolic orbit that could agree with the observations, although confined to an arc of 10 degrees. I gave at the same time the elements of a circular and of an elliptic orbit, and I showed the great uncertainty that necessarily remains when the elements are deduced from so small an arc.

Having received a more exact copy of these observations, ^{Elements of its orbit} Mr. Olbers endeavoured to determine from them the elements of an elliptic orbit; but he found so much uncertainty that he was obliged to prefer a circular orbit, since he thought it impossible to determine if the planet was near its aphelion, or its perihelion. I had proceeded on the former supposition; Mr. Gauss preferred the latter, and endeavoured at the same time to accommodate his calculations to all the observations of Mr. Piazzi: and this he performed with a difference of only a few seconds. These are his elements:

Epoch of 1801	-	-	2s [17?] ^o	36'	34''
Aphelion	-	-	10	26	27 38
Node	-	-	2	21	0 44
Inclination	-	-	10	36	57
Greatest equation of the centre			9	27	41
Heliocentric and tropical diurnal motion			12	50.914	
Mean distance 2.7673.			Eccentricity .0825.		
Revolution 1681 days, or 4 years 7 months.					

I had found the revolution 5 months and a half shorter.

According to M. Lalande's calculations, Mr. Gauss's elements give the longitude greater by a degree than Mr. Olbers's observation: according to Mr. von. Zach, my elements give it four degrees less, and Piazzi's, ten degrees less than the observation.

The idea of searching for this planet among the immense ^{give the power of searching for the planet in the collections of observations.} collection of observations of the *Histoire Céleste Française*, could not fail to present itself to all those who have attended to the subject: but it was impossible to undertake the inquiry with any hopes of success, before the elements were corrected by new observations. I shall now apply to it without delay.

Mr. Piazzi has named his planet Ceres Ferdinandia. Lalande proposes to call it Piazzi.

Some account of
M. Piazzi.

Mr. Piazzi was born at Ponte in the Valteline; and was professor at Malta, and at Palermo. When an observatory was about to be established at Palermo, in 1787, he came to Paris; he then went to London, where he procured some excellent instruments: and he has already published two volumes of valuable observations: he is now preparing to measure a degree in Sicily, and Mr. Lalande has already sent him instruments for this purpose.

Extract from Bode's Kurzer Entwurf der Astronomischen Wissenschaften. Berlin. 1794. § 387.

Numerical analogy by which Bode suspected the existence of a planet between Mars and Jupiter seven years ago.

Is it probable that Uranus, or the Georgian planet, is really situated at the utmost limit of our solar world? This appears to be very doubtful, considering the immense space interposed between it and the nearest fixed stars. Other planets perhaps may be still more remotely situated, and may perform their revolutions unseen by human eyes. We can scarcely suppose that any planet exists nearer to the sun than Mercury: but considering the proportions of the distances of the planets from the sun, we observe between Mars and Jupiter, a distance far greater than a comparison of the other distances would lead us to expect, and this space may perhaps be occupied by a planet yet unknown.

This appears to follow from a certain proportion which we find among the distances of the seven planets already known. Calling the distance of Saturn 100, that of Mercury will be nearly 4, of Venus $4 + 3 = 7$; of the Earth $4 + 2 \times 3 = 10$; of Mars $4 + 4 \times 4 = 16$; we then want a planet at the distance $4 + 8 \times 3 = 28$: the distance of Jupiter is $4 + 16 \times 3 = 52$; of Saturn $4 + 32 \times 3 = 100$; and of the Georgian planet $4 + 64 \times 3 = 196$.

Observation; by
whom?

It will, however, still be doubted by many if the conjecture quoted from Professor Bode can be thought to have been probable at the time that it was made. Calling the distance of the Earth 10, the real proportional distances are, in the nearest units, Mercury 4, Venus 7, Earth 10, Mars 15, (Ceres 28,) Jupiter 52, Saturn 95, Georgium 92. Instead of Mars 16, Saturn 100, and Georgium 196.

Lectures

Letters from Sir Henry Englefield, Bart. F. R. S. to Thomas Young, M. D. F. R. S. on the Planet Ceres.

SIR,

Blackheath, Friday.

I have seen the new planet twice, on Sunday night, and again last night. It is just visible to a common night-glass. With a power of 90 in my great telescope it was less bright than the 34 Virginis, near which it is. With a power of 200 no disc is visible, and with 300 I can scarcely say that it has a sensible diameter, more than what arises from irradiation, for small stars seen with such powers always appear dilated.

I looked at the Georgian soon after the new planet, but clouds came on, and I did not try 300 on it last night. With 200 the Georgian is, I am sure, the brighter, and it was a very much more visible object in the night-glasses.

Sunday.

Last night I again saw the new planet, and observed it with a power of 400. With this great power it seemed to have an apparent magnitude, but was extremely small, faint, and ill defined. I then turned the telescope to the Georgian (which as you know is very near), and the superiority in size and brightness was very striking. The Georgian was not well defined, but I am sure it was full four times the diameter of the new planet, and much brighter in proportion to the different size. Indeed the brightness of the Georgian is very surprising, its vast distance from the sun being considered. I really think that the diameter of the new planet cannot exceed a second; and it is of a very faint light even for that diameter. I looked then at the double star gamma Virginis, and saw the two stars distant from each other full three times their apparent diameter, a proof of the good adjustment and high power of my telescope.

I am, &c.

H. E.

VIII.

*Observations and Experiments relating to the Pile of VOLTA.
In a Letter from JOSEPH PRIESTLEY, LL. D. F. R. S.*

To Mr. NICHOLSON.

DEAR SIR,

Galvanic pile.

HAVING been favoured by Mr. Weatherby Phipson, a young man of Birmingham, with an excellent apparatus for repeating the experiments on the *pile of Volta*, consisting of sixty plates of copper coated with silver, and as many thin rolled plates of zinc, which is a valuable improvement of his own, I have had great satisfaction in observing the results; and though, receiving intelligence of what is doing on the continent of Europe so late as I do here, it is probable that I shall be anticipated in my observations, I shall lay them before you, and, with your approbation, before your readers, after observing, that I have lately received the fourth volume of your excellent *Journal*, but am ignorant of all that has been done since the publication of it.

Its admirable effects not admitted to prove the decomposition of water.

I cannot help expressing my admiration of the ingenuity with which your correspondents and others have pursued this most curious subject; and in general my results are the same with theirs, though I draw different conclusions from them, especially with respect to the modern hypothesis of the *decomposition of water*, which, though almost universally received at present, I consider as wholly chimerical, and unable to stand its ground much longer. Indeed, I perceive that doubts are entertained concerning it by several of your correspondents, and others observe that these experiments give no support to

For the oxygen does not bear the alleged proportion to the hydrogen;

it. To me it is evident that they are far from doing so. For though it may happen that the inflammable air from the wire connected with the silver end of the pile, be in the proportion to the dephlogisticated air from the wire connected with the zinc end, which that hypothesis requires, it appears that the latter comes from the air that is merely held in solution in the

but arises only from air dissolved or absorbed by the water.

water in which the process is made; since if, by means of oil upon the water, or a vacuum, access to the atmosphere be cut off, the whole production of air ceases. There is also no

The air ceases to issue, if the at-

production of air when the water has been exhausted of it; and certainly no good reason can be given why, if the water itself

itself consists of these two kinds of air, and this process be capable of decomposing it, air should not be produced from it in all these cases; both the constituent parts of the water being present, and the power of separating them being in full operation. Besides, I find that though the two kinds of air be produced, they are not always in the proportion required by the new theory, the dephlogificated air being much less than is requisite. I have also found it not much better than atmospheric air. The inflammable air I believe to be of the purest kind.

mosphere be excluded by oil or a vacuum.

If this inflammable air come from the decomposition of the water, the water from which it is extracted ought to contain an overplus of oxygen, either in the form of dephlogificated air, or of acid. But the signs of acidity bear no proportion to the quantity of inflammable air produced, and can hardly be perceived at all. I did perceive it when I made the process in water tinged with the juice of litmus, but only by the redness of the froth from the wire connected with the zinc end of the pile, the liquor itself remaining unchanged, notwithstanding a copious production of inflammable air from the other wire. Also, when I introduced a piece of raw flesh instead of the metal connected with the zinc end of the pile, no air came from it, nor did I perceive that the surface of it had acquired any acidity, though inflammable air was produced in great plenty from the other wire.

Other facts.

But, except gold or platina be connected with the zinc end of the pile, there is seldom any production of air from that quarter, the metal in that situation being dissolved; and there is no appearance of its being dissolved by any acid, but, on the contrary, of its being supersaturated with phlogiston. But before I produce the evidence of this, which affords an argument against the decomposition of water that appears to me to be perfectly decisive, I shall relate some circumstances concerning this solution of metals, which I do not find to have been noticed by others.

The suspended metal supposed to be supersaturated with phlogiston.

In general, wires connected with the zinc end of the pile are dissolved, but none so readily as those of silver, even when the wires connected with the silver end of the pile are of the same metal, and give air copiously; but if zinc or iron be connected with the silver end of the pile, any other metal, except gold or platina, connected with the zinc end will be dissolved.

Metallic solutions by galvanism.

held. dissolved. Iron and zinc were the means of dissolving each other. But the surest method of producing this solution of metal was by connecting charcoal with the silver end of the pile. I once dissolved pure gold in this manner, and I preserve the solution as an evidence of it; but I could never do it a second time, though I tried charcoal in several states, perfect and imperfect, &c. nor could I by this process dissolve platina.

Charcoal not dissolved, &c.

Charcoal itself is not sensibly dissolved in this process, and air comes from the pieces connected with both ends of the pile. Suspecting that this air might be that which always comes from charcoal when it is plunged in water, I filled the pores of two pieces of it with water, by means of the air pump, leaving them a long time *in vacuo*. Being then tried, they gave no air of some hours, but from the piece that was connected with the zinc end of the pile there proceeded a white cloud, which filled part of the vessel of water. This, however, soon disappeared, the water becoming transparent again; and after some hours both the pieces of charcoal gave air as copiously as any of the metals had done, and continued to do so as long.

iron; zinc; copper.

When iron was connected with the silver end of the pile, and copper with the zinc end, the latter was dissolved, but not till after two or three hours. Zinc being connected with the silver end, and copper with the zinc end, the former gave air copiously from the beginning, but it was near two hours before the copper began to dissolve, which (being a flat piece) it did at the corners and edges only, and never from any part of the flat surface. When I added more copper, it began to give air without dissolving, and also some of the green precipitate, which had been formed before, gave out air, and, the bubbles adhering to it, it rose from the bottom of the vessel to the top. This precipitate from the copper, which at first was green, became afterwards of a dark brown, as if the metal had been revived. This too was the case with one vessel in which a solution of silver had continued some time. It has given a coating to the glass that is perfectly white and brilliant.

four vessels of water, with wires forming the circuit, the pheno-

Having introduced four vessels of water between the two ends of the pile, and having connected each two with silver wires, that leg of the wire, in all the vessels, which was next to

to the silver end of the pile gave inflammable air, while, in all of them, the other leg of the same wire was dissolved. When I covered one of these vessels with oil, the production of air and the solution of the wires ceased in them all.

Though, as I have observed, there was a slight appearance of acidity in the water when dephlogisticated air was given out at the wire communicating with the zinc end of the pile, there never was the smallest appearance of it when the metal was dissolved. When silver was dissolved in water tinged with the juice of litmus, and there was a copious production of inflammable air from the opposite wire, I could not perceive the least change of colour in the water.

I examined the water in which the process was made, especially when silver was dissolved in it, but was so far from finding the air contained in it more pure than before, that it was evidently less so. Before the process, the standard of this air, with an equal quantity of nitrous air, was 1.1; with the water made turbid and white with the solution of silver, it was 1.2; and after standing till it became black, it was 1.3.

The black matter from this solution of silver did not contain any oxygen, but was evidently the metal superaturated with phlogiston; for when it was heated in dephlogisticated air, it diminished it, and converted part of it into phlogisticated air; and when it was heated in inflammable air, it added to the quantity of it, and this appeared, by its explosion with dephlogisticated air, to be as pure as other inflammable air; so that this black powder of silver is similar to the black powder of mercury made by agitation in water, which I have shewn to be mercury superaturated with phlogiston. Where, then, is the oxygen that ought to be produced in great quantity, if the inflammable air from the wire connected with the silver end of the pile came from the decomposition of the water?

The glass vessels in which silver has been dissolved in these proportions are tinged of a dark colour, which no acid, nor any other menstruum that I have applied, will take out. This is similar to the case of flint glass becoming black by heating inflammable air in it, the calx of lead in the glass uniting with the phlogiston of the air. In this case, therefore, it is natural to infer that this calx of silver imparts phlogiston to the glass, and that there was nothing of oxygen in it. The surface of this black powder of silver long exposed to the air while it is moist

mena ceased, when one of them was covered with oil.

No acid found when silver was dissolved.

The air in the water was worse than common.

The black powder of silver contains hydrogen, &c.

The glass vessels are blackened, as with hydrogen.

moist becomes white, which is similar to the experiment with mercury, in which the black powder of this metal, produced by the agitation of it in water, becomes white running mercury as it becomes dry, diminishing and phlogisticating the air in which it is confined.

Experiment to shew that the black powder of silver, treated by nitrous acid, contains no oxygen.

The black calx of silver, made by its solution in nitrous acid, is said by Mr. Macquer, in his Dictionary, to be owing to the phlogiston contained in that acid. To ascertain this, I dissolved some pure silver in spirit of nitre, and evaporating the solution, I heated the residuum in thirteen ounce measures of dephlogisticated air, of the standard of 0.82, with two equal quantities of nitrous air, by which it was reduced to eleven ounce measures, of the standard of 1.72, with one measure of nitrous air; so that it was almost wholly phlogisticated air. Consequently this calx of silver contains no oxygen.

Theory. That in the oxidation of the zinc there are plus and minus states of phlogiston produced in the apparatus, and that phlogiston is electricity, or nearly so.

My present opinion concerning the theory of this curious process is as follows. Since the operation wholly depends on the calcination of the zinc, which suffers a great diminution of weight, while the silver is little affected, and all metals lose their phlogiston in calcination, what remains of the zinc in a metallic form in the pile, and every thing connected with that end of it, is superfaturated with phlogiston, while the calcined part, and every thing connected with that end of the pile, is deprived of it. The former, therefore, is in a *positive* state, and the latter in a *negative* one, with respect to phlogiston; and it seems to follow from these experiments, that this is the same thing with positive and negative electricity; so that the electric fluid and phlogiston are either the same, or have some near relation to each other. The silver seems to act principally as a conductor of electricity; for the surface of it is only blackened in some places in this process, in consequence probably of receiving phlogiston from the zinc; but the water is most essential to it, because it constitutes the principal part, if not the whole, of the addition of weight in the calx. Accordingly, in the calx of zinc I have found nothing but water, though it is probable that there is a small portion of oxygen in it.

or two electric fluids, oxygen and phlogiston.

These experiments favour the hypothesis of *two electric fluids*, the positive containing the principle of oxygen, and the negative that of phlogiston. These united to water seem to constitute the two opposite kinds of air, viz. dephlogisticated and inflammable.

These

These experiments tend likewise to confirm the conjecture ^{Muscular motion.} which I advanced in my first publication on the subject of air, concerning *the similarity of the electric matter and phlogiston*; and, together with the proper galvanic experiments, shew, that the same substance elaborated from the aliment by the brain is the cause of muscular motion, the nerves being the most sensible of all electrometers. See the first edition of my *Experiments on Air*, vol. I. p. 274, &c.

I see no occasion to suppose, with Mr. Volta, that there is ^{No circulation of electricity supposed.} any circulation of the electric fluid in this pile. The calcination of the zinc supplies phlogiston as long as it continues, and when that ceases, the operation of the pile ceases with it. I also see no necessity that one end of the pile should be silver and the other zinc; and when both are silver, or both zinc, the operation is the same, nor can I conceive why it should be otherwise. When the pile is properly prepared, the addition of any kind of metal to the ends only serves as a conductor of the electric fluid; and silver, zinc, or any other metal, will sufficiently answer this purpose.

Had this process succeeded without any ^{Remark.} atmospherical air incumbent upon the water in which it is made, it would have amounted to a full proof of the new theory, one part of the water being deprived of hydrogen, while oxygen abounded in the other, and both of them with the assistance of *caloric*, (though it does not appear whence that could be supplied), assuming the form of air. But this not being the case, the element of the dephlogisticated air evidently coming from the superincumbent atmosphere, the element of the inflammable air must necessarily come from the calcined metals which is a sufficient proof of the doctrine of phlogiston. Whether in this you will agree with me or not, I am,

Dear Sir,

Yours sincerely,

J. PRIESTLEY.

Northumberland, in Pennsylvania,
Sept. 16, 1801.

P. S. In your *Journal*, vol. IV. p. 226. it is said, "the inventor of the galvanic pile discovered the conducting power of charcoal;" whereas it was one of my first observations in electricity,

electricity, made in 1766, and published in the first edition of my *History of Electricity* in the year following. See that edition, p. 598.

Experiment of the pile under a receiver, which completely deprived the air of oxygen.

Second P. S. After the above was written, I covered the whole pile with a large receiver, standing in water, charcoal being connected with the silver end of the pile, and silver with the zinc end, in two vessels of water; when the solution of the silver took place in both the vessels, and the air within the receiver began to diminish. The diminution having come to its maximum in about a day and an half, I examined the air within the receiver, and found it completely phlogisticated, being not at all affected by nitrous air. There was evidently, therefore, no dephlogisticated air generated, the whole result being the effect of the calcination of the zinc. This experiment, added to that on the black *calx of silver* produced in this process, and on the *water* in which it is made, completes the proof of there being no decomposition of water in this case, and strengthens the argument in favour of the doctrine of *phlogiston*.

IX.

Experiments and Observations towards determining the Influence of Oxygen on Germination. By Dr. CARRADORI.*

History of the subject. Authors: Boyle, Homberg, Muschenbrock, Boerhaave, Achard, Ingenhousz, Humboldt, Decandolle.

IT was long ago ascertained, by the experiments of Boyle and Homberg, that seeds require air for their germination, for it was proved that they do not germinate *in vacuo*. This necessity of air was afterwards confirmed by the experiments of Muschenbrock and Boerhaave, who had supported the observations of all times by the remark, namely, that seeds buried very deep under ground do not germinate, and that a great number of seeds do not even germinate under water, but perish in that situation. When this subject was afterwards studied with greater attention, in order to ascertain the reason why air is indispensable in this operation, it was found to be necessary in consequence of the portion of vital, or oxygen air which it contains: For Achard in the first place, and after

* Journal de Physique, LIII. Vendemiaire. 10.

him other philosophers, have shewn that germination does not take place in any mephitic air; as, for example, inflammable or hydrogen air, phlogistic or azotic air, which are known to contain no oxygen; whence it was necessarily concluded, by a legitimate inference, that of all the atmosphere which we term the air, the oxygen is the only portion necessary to germination. This has been confirmed in a more direct manner by Ingenhoufz, who discovered, that the greater the abundance of oxygen the greater is the rapidity and facility of germination in grains or feeds; which probably led Humboldt to the discovery of the means of accelerating germination, and effecting it even in the most obstinate feeds, by the oxygenated muriatic acid, or even, as Decandolle pretends, of the nitric acid, which are known to be charged with oxygen.

At present it remains to be ascertained what influence oxygen exerts upon growing feeds; that is to say, what precisely are its effects upon germination. In order that germination may take place, it is necessary that there should be a principle of fermentation in the farinaceous substance which resides in the cotyledons or placenta of the feeds, which substance, as every one knows, envelopes and surrounds the germen or embryo; hence there is reason to believe that this substance, combining with oxygen by means of fermentation, acquires a degree of acidity, and consequently a stimulus proper for exciting the vitality of the germen, and for giving the first impulse to the circulation of the fluids in the embryo; or that, by means of the oxygen, this substance becomes modified so as to acquire particular properties, by which it is rendered capable of affording the first nourishment to the tender fœtus or embryo of the plants; since it has been demonstrated, by the late experiments of Dr. Rollo, that in the germination of the farinaceous feeds, the amylaceous part is converted, by means of the concurrence and combination of the oxygen, into saccharine substance, or even both.

It is not certain, however, that oxygen influences this only; that is to say, that these are its sole effects: in order therefore to ascertain this point, I made the following experiments.

I placed feeds or grains of wheat, *tritium*, in a plate with water, in the following manner: I fixed them to the bottom of the plate with soft wax, so as to make them remain in a perpendicular or straight direction, some with the germen upwards, The seeds of wheat germinate most speedily when the germen is in contact with oxygen.

wards, others with the germen downwards, and so that the water reached precisely to the level of their height; that is to say, that it did not entirely cover them, in order that one of their extremities might be in contact with the air. The season was favourable to germination, it being the month of September, when the thermometer indicated 17° Reaum. or about 21° of the thermometer of 100 degrees. On examining them twenty-four hours afterwards, I found that those which had the germen upwards, or in contact with the air, germinated remarkably well, though all the rest of the substance of the seed was immersed in the water; and that the others, which had been placed in the water with the germen downwards, though one of their extremities was equally in contact with the air, namely, the extremity opposite to the germen, could not well germinate; they merely exhibited some signs of germination; and there was no difference to be perceived between those grains and others which I had placed on their side, not upright or perpendicular to the bottom of the plate, but lying down horizontally, and consequently covered entirely with water. Thus it appears that perfect germination does not take place unless when the part which contains the germen is in contact with the air, although all the rest of the seed be guarded against its immediate contact.

Sprouted wheat grew well where the developed plant touched the oxygen; but did not when it was covered.

I took seeds or grains of wheat which had already germinated in the open air, and by means of the same wax I placed them in the water, some with the germen upwards, others with the germen downwards, in the same vessel, and with the same precautions, namely, not suffering the water entirely to cover them; so that those which had the developed germen, or the small plant directed upwards, were in contact with the air at this point only. When I examined them again, I found that those which had the germen downwards, and which consequently were not in contact with the air at this part, remained as they were, and exhibited no other sign of further germination; whilst in the others, whose germen was exposed to the air, the germination advanced, and the small plant increased remarkably in growth.

The stalk is the part where the oxygen ought to apply.

I afterwards wished to ascertain, if it were indifferent whether all parts of the germen were in contact with the air, or only some of its parts. For this purpose, I chose a number of grains of wheat which had germinated well for twenty-four hours

hours preceding, some of which I placed in the water with only the stalk projecting out of it, whilst all the remaining part was immersed under its surface, and others with the stalk under water, and the small roots above its surface. I did not find that the germination made any progress with those which had their stalk under water; but those whose stalks were in contact with the air thrived and advanced in growth, and in a few days time the plant was developed.

Thus oxygen, or vital air, is necessary to germination, since the tender plant cannot thrive and grow without its immediate contact. Thus we see, that though it has received life by the effect of germination, it cannot preserve it unless it be in contact with the air at this part; that is to say, at the stalk, which possesses an organisation adapted to receive its benign influence, which is necessary to the support of life. The germination commences without the immediate contact of oxygen, but it cannot become vegetation, if I may so express myself, without its immediate contact. It seems, that in order to animate the germen or embryo of plants, and give it life, by means of the circulation and other functions, combined oxygen is sufficient, but that it cannot afterwards subsist, if this element in a free and pure state is wanting to the tender plant.

Germination may begin without free oxygen; but vegetation cannot proceed without it.

In fact, as I have already remarked, and as I have observed still better in the subsequent experiments, the seeds begin to germinate under the water, but their germination ceases and proceeds no farther, unless the air receive the plant already born, or the animated germen. But germination is not effected under water unless in a certain case.

I selected a number of good seeds or grains of wheat; some of which I put into a plate, and poured in as much water as was precisely necessary to cover them; others I put into a deeper vessel, and covered them with a larger quantity of water; others I put into a glass, and covered them with a large quantity of water; and, lastly, I put others into a bottle with a narrow neck, and covered them so that the water reached to the neck of the vessel. After twenty-four hours, I found that the seeds in the first and second vessel had begun to germinate, but none of those in either of the two last vessels germinated, nor did they afterwards.

But no germination takes place unless the water be exposed to the air, &c.

under circumstances favourable to its absorption of oxygen.

Hence it clearly appears, that as this difference can be referred only to the diversity of the vessels, that is to say, to the different surface which the vessels in which the seeds had been placed expose to the air, and to the different quantities of water in which they had been immersed; or, in other words, to the difference of the action or influence exerted by the oxygen contained in the atmosphere upon the water: germination cannot even commence without the immediate contact of vital or oxygen air; provided, however, that the water in which the seeds are immersed be in a condition to become changed with the oxygen, which is necessary to effectuate such a function. It cannot be doubted but that water powerfully attracts a large quantity of oxygen from the air; it has a strong avidity for this principle, and becomes abundantly loaded with it. M. de Gleichen and Senebier had observed that the seeds of pease, when placed in too large a quantity of water, perished without germinating, but they had given no accurate explanation of the fact.

Recapitulation.

I have thus established two essential facts relative to germination, which, to the best of my knowledge, had not before been pointed out. Vital or oxygen air is necessary to the grand process of germination; but in order to give the impulse or the principle of this germination the immediate contact of the air is not necessary, but it is indispensable to its continuation or progress; since the germen already animated, or the small plant, cannot grow nor vegetate, unless it be in a state to enjoy the immediate influence of this vital fluid. Ingenhousz had only asserted in general terms, that the contact of oxygen is necessary to the germination of grains or seeds; but something more particular was still wanting.

Repetition of experiments, with other plants.

I have repeated the same experiments upon the seeds of barley, *hordeum vulgare*; of beans, *vicia faba*; lupines, *lupinus albus*; and have obtained the same results: hence we may conclude that the above-mentioned inferences are applicable to the seeds of all *terrestrial plants*, that is to say, such as grow themselves upon the dry ground.

Seeds of aquatic plants germinate and grow by the combined oxygen of water.

As to the seeds of aquatic or marsh plants, it appears, that, both for the commencement and for the continuation of germination, the oxygen which is combined with the water is sufficient, without the concurrence of free oxygen, as we see that they germinate, and afterwards grow very well, under the

the water. Their organisation, different from that of the terrestrial plants, enables them to avail themselves, for the same operations, of the oxygen which is dissolved in the water.

Though seeds can germinate under water, yet by being kept there they lose much of this faculty, and at length become altered to such a degree as to be no longer capable of germination, even though we change their place, and transport them into dry earth. Seeds are injured by lying in water.

I have observed that seeds of wheat, that had been kept in water during three successive days, but not under circumstances to enable them to germinate, and were afterwards transported into dry ground proper for producing this effect, germinated almost with the same vigour as if they had been placed there at first; but the fifth day the greater part ceased to germinate, and those which still germinated did it in a languid manner, and the small plants which they produced were sickly, and without vigour. Experimental proof,

I have remarked, that the same happens with plants which have germinated under water; by remaining there during too great a length of time, these tender plants are spoiled, and no longer capable of growing and thriving, even though they change their situation, and are placed under more favourable circumstances. I have also observed, that this too long abode in the water is the more dangerous to them when these plants are tender; that is to say, nearer to the period of their birth, or commencement of germination. even after germination.

Hence we see what danger arises from too long continued and abundant rains after the seed time. The too great quantity of water keeping the seeds for too great a length of time in a state of submerision, prevents their germinating, or injures their healthy germination, and is the cause why the plants which they produce appear with a constitution more or less altered, in consequence of the prevention of their communication with the air, or, in other words, for want of oxygen. Hence the wet winters, which, for the reasons that have been mentioned, may prove dangerous to the seeds of wheat, the most important of the fruits of the earth, are justly considered as prefiging a scanty harvest. Hence the mischief of abundant rains after seed time.

—————Hyemes optate serenas
Agricolæ: hyberno lætissima pulvere farra
Lætus ager—————

VIRG. GEORG.

Dry windy winters are best for the seed.

Dry windy weather is then advantageous, since the oxygen necessary for the grand work of the germination and growth of the tender plants is furnished easily and abundantly by the atmosphere. After this period, that is to say, after their infancy, the plants no longer require oxygen, in order to prosper, but a mephitic air, as I have already shewn in my memoir *On the Fertility of the Earth*,* because in this they find their principal nourishment: thus when the period has elapsed, oxygen is but of very limited utility to vegetation.

X.

On the Choice and Use of a Razor. By a Correspondent.

To Mr. NICHOLSON.

SIR,

January 26, 1802.

IF you think the following observations applicable to your paper on Shaving, you will insert them in some future Number of your Journal.

W. B.

On the choice of a razor.

In choosing a razor (besides the marks you enumerate) prefer that blade whose edge is least blunted or turned after being two or three times drawn upon its edge, from heel to point, on a bit of horn or the thumb nail.

Strapping.

In strapping care should be taken to give the finishing strokes in the direction in which it is about to be used: that is, if the drawing stroke in shaving is made from heel to point, the razor should be drawn from heel to point upon the strap, and *vice versa*. This observation applies to all fine edges, especially to surgical instruments. For a strong beard, a rounder edge is given by a loose strap; but a keener and more durable one is given by a strap fixed upon some inelastic substance.

* Della Fertilita della Terra. Memoria del Doct. J. Carradori, Premiata della R. Societa Economica di Firenze nel mese di Giugno del 1799.

The soap lather, besides the effects already mentioned, The lather, seems to act by giving a degree of firmness to the beard. It unites, in some measure, a great number of hairs, and thereby occasions a more equal resistance to the stroke of the razor, diminishing the elasticity of the hair; and also taking off a little of its natural fine polish. In shaving without soap the beard is generally irregularly cut, especially if the edge be not perfectly keen; as may be observed in cutting a bundle of bristles loosely tied, or taking the edge off a quire of paper without putting it into a press. Provided the lather be of sufficient consistence, it is not material whether hot or cold; for I suspect the alkali acts pretty quickly upon the polished surface of the hair.

The form of the razor is of some consequence. Young The form of the shavers, who may not be equal to giving the drawing stroke, blade. should choose a scimitar-like blade. Every razor ought to have the point terminated by a segment of the circle; otherwise it is difficult to give the stroke from the point towards the heel, which is often necessary when a man is not *ambodexter*.

Another Correspondent has favoured me with what seems Effect of heat to be the most probable explanation of the effect of heat on on edge tools. He observes, that in the cold regions of North America an axe will sometimes fly in pieces like glass, and that our smiths in this country are well aware of the increase of tenacity which a very slight increase of temperature gives to steel, iron, and other metals, and take care to use it when they set or alter the figure of any tool or utensil by cold hammering. They warm the article to prevent its breaking. He thinks the fine edge of a razor, which would splinter and become rough if strapped or used cold, may perform its office much better when rendered more tenacious by a moderate heat.

W. N.

XI.

*Observations on the Method of painting with Milk. By
CITIZEN DARCET, Member of the Lyceum of Arts, and
Essayer of Money.**

Importance of
substituting caseous
matter for
size in painting.

UPON reading the different articles of the Decade Philosophique, in which the method of painting in milk is mentioned, and on examining the theory which Cadet-de-Vaux has developed respecting this useful application of our chemical knowledge, I perceived how important it would be if we could substitute the caseous or cheesy matter, which we possess in such great quantities, to glue or size, which is commonly used in painting in distemper, and by this means to appropriate that substance to paper-hangings and other arts, for which it is an article of the first necessity. I apprehended that the process of this new method of painting was susceptible of being rendered more simple, and I endeavoured to study the different phenomena in order to ascertain what substance might be left out in part, or even wholly, without altering the goodness of the colour. This examination has enabled me to make the following observations, which may serve as a supplément to the memoir published on that subject. †

Historical remarks.

I shall not attempt to solve the question, whether painting in milk was known to the ancients. The solution of this problem, which is perhaps impracticable, is scarcely interesting to us. I shall only remark, that the Indians, who used milk to dilute the colours with which they painted the sides of their cabins, appear to have given the first notion of the application of a natural mixture of the caseous part along with the serous part, to render colouring matter adhesive. ‡

For this application we are indebted to Cadet-de-Vaux, who has by these means afforded a real service to the public,

* Decade Philos. No. 5, An. X.

† See Philos. Journal, quarto, V. page 217.

‡ Our house painters are also acquainted with the advantageous uses of milk in inside paintings. They have long used a mixture of milk and well washed lime to give a brighter white to delicate objects in relief, in order that they might appear more prominent, from the ceiling painted with the ordinary white-wash.---D.

by

by rendering a material useful which in many places had, properly speaking, little value, unless in the ordinary use of extracting the butyraceous part, which alone represented the whole value of the milk before its decomposition. He has at the same time given a degree of perfection to this paint, by rendering the process sufficiently simple, to afford a solid colour, nearly without smell and at a moderate price.

These qualities, which painting in milk really possesses, appear to render it little susceptible of amelioration. I shall nevertheless venture to propose one which has succeeded in my hands, and of which the experiments have been made on a scale sufficiently extensive to insure the goodness and the value of the results I have obtained. *

After having given an account of the observations which led me to this process, I shall speak of the materials I used, their proportional quantities, the best method of combining them, and, in a word, the process to which we must justly apply the denomination of cheese painting. †

Citizen Cadet-de-Vaux, in his memoir on milk painting, has given two processes, which, as it appears to me, ought to be considered as one, because the first which he announces as similar to common distemper does not differ from the second, which he gives as proper to be substituted instead of oil painting, except in the Burgundy pitch, which constitutes part of this last. In fact, Burgundy pitch renders the colour more solid, but it is far from rendering it capable of being washed like oil paint; a property which is known to be truly characteristic of this paint, and is of itself sufficient to distinguish it from distemper. I shall not, therefore, examine these two processes separately; and I shall conclude my ob-

Cadet-de-Vaux's two processes do not differ. Critique on his method.

* I painted an entire chamber with the colour I shall proceed to describe. Individuals, who slept in it the very day of the operation, were in no respect incommoded by the smell of the paint, though the door and the windows were shut the whole night.

† We find, in the *Dictionnaire de Peinture de Pernety*, a note in which he speaks of a pamphlet entitled, *La Peinture au Fromage ou au Ramekin*. It was written in opposition to the encaustic method of painting, of which Citizen Bachelier has, as it were, re-invented the process. I have not been able to procure this pamphlet, for which I am very sorry, as it might probably contain some data respecting the method of painting on which we at present treat in a more serious manner,---D.

ject to be attained, if the process I offer shall afford a colour equally good as that which is obtained by following the process for the resinous milk paint.

We read at page 250 of the memoir to which I allude, that the oil added to the mixture of flaked lime and skimmed milk is dissolved by the lime, and then forms a calcareous soap.

Lime, milk, and oil form a triple compound.

By carefully examining what passes in this operation, I have thought I observed that the lime does not separate from the caseous part to combine with the oil; but that the oil which is added in the mixture forms a triple combination, little soluble in fact, but perfectly diffusible in water. We know, on the contrary, that calcareous soap is insoluble and quite immiscible in water; and I have found that the addition of skimmed milk produces no change in these properties. I have also remarked, that the triple combination does not take place, but in the order described by Cadet-de-Vaux, for the lime does not entirely combine with a mixture of oil and skimmed milk. In this case there is only a formation of calcareous soap, which remains in masses suspended at the surface of the liquid.

Sour milk unfit for the pigment.

Cadet-de-Vaux also announces, in page 248 of his memoir, that sour milk is no longer proper for the composition of this colour. He observes, that the serous part of the milk being converted by fermentation to the state of acetous acid, may then form a kind of calcareous acetite, which, by its deliquescence, must contribute to destroy the colour in which it is mixed.

The whey is of little utility in the paint; but very useful in many other applications.

I have verified this fact, and I think that the serous part cannot be useful in the composition of paint, unless before that period in which the acetous acid shall have converted the sugar of milk it contains into acetous acid. For in the former case it is capable of giving solidity and brightness. But when we consider that the fermentation which produces this phenomenon takes place with much rapidity; that in many countries this serous part is used in bleaching, in the preparation of sugar of milk, and for the feeding of animals, we shall be desirous of retrenching it from a process in which it is sometimes useful, but oftener noxious, in order to appropriate it entirely to these several uses.

By

By examining, in the same manner, the effect of the addition of oil upon the rest of the mixture, we soon perceive that if it does not improve the solidity of the colour, it must either be useless or pernicious; because it renders the tint dull, and communicates a disagreeable smell. Now experiment demonstrates the negative, for milk paint in distemper does not resist water more than the simple mixture of skimmed milk with slaked lime and whiting.

The oil is of value as an ingredient.

I have also concluded that the quantity of six ounces, or 183,430 grains of slaked lime, prescribed in the recipe for painting in distemper with milk, was much too considerable, because two ounces, or 61,143 grammes, or even a smaller quantity, is sufficient entirely to liquify one French pint, or or 951,206 cubic centimeters of skimmed milk. The pellicle of calcareous carbonate, which is formed after a time at the surface of painting in milk distemper, shews, as well as other experiments on this subject, that the present observations are well founded.

The proportion of lime is too great.

With regard to the whiting, I think the dose may be increased without inconvenience. Cadet-de-Vaux already added three pounds, or 1467,438 grammes, to five pounds, or 2445,75 grammes. I have gone further, and have used a dose of eight pounds, or 3913,168 grammes, and the colour I obtained did not appear to me less solid than that made according to the original recipe. This observation does not apply to the preparation, when required to be coloured either by an ocre, an earth, or an oxide. I have remarked, that these substances require more of the mordant than the whiting does; and this happens, in fact, with painting in distemper with milk coloured yellow or red, in order to be applied to pannels, &c. namely, that the addition of these colouring matters deprive it of all its solidity. We must not, therefore, add the colour to the paint entirely mixed, as is directed at page 248 of the memoir, but diminish the dose of whiting in proportion to the quantity of colouring matter necessary to produce the required tint; in a word, it is requisite that the colouring matters added to the whiting should not weigh at most more than the five pounds, or 244,573 grammes prescribed in the recipe.

The whiting may be increased.

Colour must be added during first mixture.

These are the principal observations I have made on painting in milk; let us now see what are the consequence we may justly derive from them.

I have

an improved
manipulation.

I have concluded that the oil or Burgundy pitch, the ferous part, and a portion of flaked lime, might be entirely left out of the recipe. My paint will, therefore, contain only the caseous part, a portion of flaked lime and whiting. Reasoning pointed out these conclusions; but my doubts on the subject were not yet cleared up by experiment.

I formed a great number of mixtures by constantly varying in each of them, in opposite directions, the doses of cheese, of lime and of whiting. Many of my experiments were useless; but I succeeded in discovering the following process, which appears to me to answer the intended purpose. The following are the doses:

Proportion of ingredients.

Cheese or curd well drained	144 grammes	=	5 oz. avoird.
Slaked lime	7 grammes	=	$\frac{1}{4}$ oz.
Whiting	280 grammes	=	10 oz.
Fine powdered charcoal	2 grammes	=	1 dram.
Water	80 grammes	=	3 oz.

The kind of cheese.

This cheese is commonly called *fromage à la pie*, or soft cheese. I have used old cheese which was almost dry, which nevertheless afforded good results, but the fresh cheese is certainly to be preferred. I must also observe that these cheeses differ from each other; they are not all equally proper for the purpose, and I have found some which compose colours of little solidity; they were, in general, disagreeable to the taste.

Manipulation.

Let us now speak of the manipulation. At the moment of commencing the operation, a certain quantity of strong quick lime must be slaked in the least possible quantity of water. This is the surest and most speedy method of reducing it into fine powder. The lime is to be sifted, in order to separate the pieces which do not fall down, and of the powder seven grammes are to be weighed. The quantity of cheese above indicated is to be taken and pounded till it has the appearance of salve, and with this the seven grammes of lime are to be mixed, and the mixture well agitated, which loses its consistence, and acquires that of hot new made glue.

On the other hand, whiting in powder is taken, and added to the water and the charcoal, and the whole accurately mixed. This mixture may be passed through an open sieve, in order that it may be reduced to a liquid homogeneous paste.

The

The mixture of lime and cheese is then to be added, and carefully mixed with that of the whitening and charcoal diffused in water. The colour is then finished.

The doses here pointed afford a colour too thick to be used Dilution, in this state. We must therefore add to the mixture a quantity of water necessary to communicate the degree of fluidity desired; but this addition must not be made till the moment before it is used, for I have observed that the colour keeps better the less water it contains.

Two hundred and ten grammes of water added to the Surface covered. colour, made as before prescribed, afford the necessary quantity for exactly covering a square surface of 1,948 meters, or one toise or fathom. It may, however, be easily apprehended that the doses of water, and even those of charcoal, may be varied to a certain point, according to the judgment of the operator.

When a red or yellow colour is desired, similar to that Colour. which is used for pannels, ceilings, &c. I substitute, instead of whitening and charcoal in the foregoing process, the colouring matter which I intend to use. The following are the preparations which appeared to me to afford the most solid colour most capable of supporting the encaustic and wax (*l'encaustique et le cirage.*)

Well dried curd or cheese	- - - - -	144 grammes
Slaked lime	- - - - -	7 grammes
Colouring matter	- - - - -	200 grammes

If, instead of the ocre, we substitute charcoal in a state of Shining effect by high division, lamp black, for example, we obtained a black wax or sugar. colour, which may be used with success to blacken the leather of trunks.

When the colour is dry, if it be desired to give it a shining appearance, it is covered with two coats of a solution of white wax in the essential oil of turpentine. When this kind of encaustic is dry, the wax may be polished by friction with a clean cloth. This preparation has the advantage that it does not scale, but resists water a little, properties which the compositions usually applied to this purpose do not possess.

If, to the mixture of cheese, lime and lamp black, a small quantity of dried sugar or honey be added, a black colour is obtained, which dries speedily, and is sufficiently shining to

be used like the foregoing. The experiments I have made on this object, though satisfactory, are not yet perfectly equal to my wishes.

The paint is mixed according to the preceding instructions, and the necessary degree of fluidity for painting a first or second coat is given by means of water, the dose of which must be determined by circumstances and judgment. The whole is then covered with the common encaustic, and the operation is finished as usual by rubbing.

Advantages of
this new paint.

I shall not enlarge more amply on the utility of the paint which I here propose. Every observation which has been made by Cadet-de-Vaux may be applied to this, because the various changes I have made in this process do not deprive it of any of its advantages; but, on the contrary, render it more valuable. The solidity of the painting by means of cheese is at least equal to that of painting with milk and resin; but the former is more beautiful, less disposed to become yellow, is more simple, and must necessarily cost much less than the paint in distemper with milk, and still much less than the resinous paint with milk. Water does not spot it, nor even leave any trace after it is dried, an advantage which size painting is very far from possessing.

It may be kept
very well, or
made into cakes.

This paint with cheese may be kept very well, particularly when it does not contain much water. I have several times formed it into cakes, which, when pounded after their entire desiccation, formed, by the addition of a very small quantity of cheese and lime, a colour as solid as that which had been newly made. This experiment shews how much the export of this colour manufactured in agricultural districts may become advantageous in the way of trade.

How far this or
any other paint
can be a pre-
servative against
infection.

With regard to the property which the resinous paint with milk possesses of depriving walls of infection by its application upon surfaces penetrated with putrid exhalations; as this effect is purely mechanical, we may by analogy infer that painting with cheese must answer the same purpose, because it is sufficiently adhesive and fluid completely to stop the pores of the stones, plaister, and wood, to which it adheres no less strongly than the resinous paint with milk. But this property appears to me of little real utility. The processes which Citizen Guillon has given in his work on disinfecting the

the air, may be applied to the case mentioned by Cadet-de-Vaux, and answer the purpose with infinitely more speed and perfection. It appears, therefore, that those methods only ought to be used in such cases. For the fumigations operate of themselves, and without the assistance of a workman; whereas the painter of an apartment or infected chamber must necessarily expose his health, and sometimes even his life.

It only remains, after having thus endeavoured to simplify the process which Cadet-de-Vaux first brought to perfection, that I should express my wish that the advantages it really possesses might engage the manufacturers of paper hangings to adopt its use. The inhabitants of the country, who must naturally be more strongly impressed with its advantages than those of towns, will, no doubt, be the first to avail themselves of it. This application may at least assist us in saving an annual tribute, which our manufacturers pay to foreigners for animal glue or size. If cheese could hereafter be used as a substitute in block printing, I should think myself happy to have contributed one step towards the perfection of an art no less useful than agreeable.

Suggestion that it may be used in block printing of paper hangings.

XII.

Description of an Hydraulic Bellows for a Smith's Forge. By
Mr. J. C. HORNBLOWER.

To Mr. NICHOLSON.

S I R,

I SEND you a sketch of a smith's bellows a little out of the common way. Its application to my purpose was suggested by an acquaintance I had by the large machines of that sort erected at Willy Furnace, by the late Mr. Isaac Wilkinson, father to the present Mr. John Wilkinson, about forty years since, and where circumstances or inclination have taken the lead, I have adopted the principle occasionally in this form.

Small hydraulic bellows for a forge.

What I have principally in view is, to mention to you a very striking difference between the effect of this bellows, and a common leathered 30 inch bellows in the same shop. The leathered bellows throws considerably more air to the fire, and

Its effect compared with common leathered bellows.

DESCRIPTION OF AN HYDRAULIC BELLOW.

its nozzle compared with this, is as ,73 to ,60 in diameter, but it does not produce so great an effect in bringing on the heat, and the voice of this is so great as almost to drown that of the common one. The only difference in other respects is, that in the hydraulic bellows the pipe goes underground for about eight feet, and the conducting pipe of the other comes down about the same distance from the shop above.

If any further particulars are necessary for your comment on this subject, they are at your service.

I am, SIR,

Your very obedient Servant,

J. C. HORNBLOWER.

East Row, City Road, Feb. 12, 1802.

DESCRIPTION. Plate XII.

Description of
its structure and
operation.

A. The plunger, or working part of the bellows, 18 inches square within, which receives the air by a valve in the hinder part opening inwards, which at the stroke by the rockstaff E, throws it down the tube indicated by the dotted lines, which has a valve opening into the reservoir D, whence it is led to the tuyere by the pipe P. Length of the plunger 20 inches, stroke nine inches. Diameter of P three inches; of the nozzle 0,6 inches.

The whole is placed in a pit or cistern, having water sufficient to rise to the lower end of the tube where the valve hangs; this tube is the only communication between the upper part and the reservoir D: when as much water is poured in round the working part over the wash boards, as will rise within five inches of the upper edge of them, the bellows is ready for use. The little frame work serves to keep it from rising, and affords a convenient support for the balance and the rock staff.

I should observe, that the area of the pit or cistern ought to be at least twice as much as that of the plunger A*.

* From the pressure of other business, it has not been in my power to visit this manufactory, in order to ascertain the cause of the difference here stated; but I shall give the facts more particularly in the next number.

W. N.

XIII.

On the Practicability and Advantages of a general System of Rail Roads, and the Means of carrying the same into Effect. In a Letter from RICHARD LOVELL EDGEWORTH, Esq.

To Mr. NICHOLSON.

S I R,

MANY years ago I formed the project of laying iron rail-ways for baggage waggons on the great roads of England; but having consulted several of my friends, who were eminent mechanics, so many objections were started, that I for some time despaired of success. One great objection arose from the vast expence of massive rail-ways, and the continual cost of repairs. To obviate this difficulty, it occurred to me to divide the weight that is usually carried upon a single waggon into four or five portions, and to place them upon four or five small carriages; these carriages linked together would be as easily drawn as the same load upon one waggon. In pursu-^{The public advantage of rail roads long ago stated by the author,} with models, ^{&c. to the Society of Arts; viz. in 1768.} an-
 ance of this idea, about the year 1768 I presented models of three such carriages to the Society for the Encouragement of Arts and Manufactures, who, for this and other inventions in mechanics, honoured me with their gold medal; the date of which, and the journal of the Society may ascertain the early claim which I have to this invention. In 1788 I constructed ^{Experiment in} four carriages with cast iron wheels, truly turned and supported upon friction rollers; these were shewn to several eminent persons, and were employed upon a temporary moveable wooden rail-way for a considerable time, in carrying limestone for the improvement of land. A variety of accurate experiments, and some useful improvements in this mode of carriage were made with these machines, which it would take up too much of your valuable work to detail. I shall mention only one idea, which appears to me so practicable, that I beg a place for it in your Journal, which will secure to it the most extensive circulation here, and on the Continent.

I propose, that by way of experiment iron rail-ways should be laid on one of the great roads, to the distance of ten or twelve miles from the metropolis, upon something like the following ^{Plan for the commencement of a public rail-way. Four roads; two for}

heavy carriages,
out and home,
and two for light.

lowing plan : four rail-ways should be laid on the road, raised on sleepers of stone, so that their upper surface should stand about four inches above the road. They should be made hollow from the bottom upwards, for strength and to save expence broad at bottom, and rounded at the top, to prevent the lodgment of dirt and dust. On these should run light waggons, each containing not more than one tun and a half weight.

I have mentioned four rail-ways. The two inside roads should be appropriated for waggons, and the two external rail-ways for coaches and chaises, &c. The left hand rail-way invariably to be followed by each species of carriage on its own road ; so as to prevent the possibility of any carriages meeting on the same rail-way. By appropriating the exterior tracks to light carriages, those which wished to pass others might turn off upon the waggon road, and resume their proper place

The carriages to
be drawn stand-
ing on platforms
or cradles adap-
ted to the rail.

after they had gone by the carriage they wished to pass. Now to accommodate coaches and chaises, &c. to these rail-ways, I would have them carried, wheels and all, in cradles or platforms, which should have wheels adapted to the rail-ways. By these means no alteration would be necessary in any of the carriages commonly used ; but the horses of any coach or chaise might, as soon as they had got out of town, walk up an inclined plane into the cradle or platform, and draw their respective carriages after them : the horses should then walk out at the farthest end of the platform, upon the road belonging to the rail-ways. They would then draw the chaise not upon its own wheels, but upon the wheels of the platform or cradle in which the *chaise* should be detained.

Expected rate of
travelling, 12
passengers, six
miles an hour by
one horse, &c.

For stage-coaches similar platforms should be provided, and in these six inside and six outside passengers might travel at the rate of six miles an hour with one horse. Hackney, or gentlemen's chaises might go at the rate of eight miles an hour with one horse, without interruption or delay.

Hills avoided.

Where hills intervene, new roads must be made following the course of streams that wind between the hills, a moderate acclivity would not obstruct the progress of these carriages, that is to say, a rise of one foot in ten.

Numerous be-
nefits from this
scheme.

Every person conversant in these subjects, must know how much within bounds I speak with respect to the ease of draught upon rail-ways. The saving of horses and their food, the saving of wear and tear of carriages, the increased distance to which

which horses could travel in a day, the freedom from dirt and dust, the security by night, the ease with which the sick and infirm might be transported from place to place, are all obvious considerations; but the chief convenience of this project arises from the mode of receiving and transporting on rail-ways every carriage now in use *without any change in their structure*, so that a traveller may quit and resume the common road at pleasure. To enumerate all the advantages that would arise from such roads, is unnecessary in this slight sketch; the intelligent reader will probably perceive many that are not even hinted. It is self-sufficient to lay the general idea before the public; the contrivance and economy of the different parts of such a project require long and minute details, which must be reserved for another stage of the business. If such a plan should have a fair trial, it might lead the way to farther speculations.

In this plan the carriages require no alteration.

It is not impossible by *slight circulating chains*, like those of a jack running upon rollers, to communicate motion between small steam-engines, placed at a considerable distance from each other; to these chains carriages might be connected at will, and when necessary they might instantaneously be detached. What a prodigious saving of expence might be thus effected? If the freedom and facility of intercourse, which has been obtained by good roads and canals, be, as Adam Smith asserts, one of the great causes of our national wealth, how far might this freedom and facility of intercourse be extended by the perfection of the scheme, whose outlines I thus lay before the public.

Draught by steam engines.

Every great project requires time for consideration; time accustoms the public mind to new views, and what at first appears too distant and unattainable by *time* becomes familiar and practicable.

The detail and reception of every great project requires time, &c.

Mechanics will not fail to comment on what appears in such a respectable publication as yours. You will not, Mr. Editor, hesitate to give your own opinion; you will at least be certain, that whatever objections are raised will be treated with candour, and replied to without enthusiasm, by

Your humble Servant,

RICH. LOVELL EDGEWORTH.

Edgeworth Town, Ireland.

XIV.

*Description of the darkening Apparatus for Telescopes by Means of Fluids. By WILLIAM HERSCHEL, L. L. D. F. R. S. * with an Engraving. Plate XI.*

Construction of an apparatus for viewing the sun through a coloured fluid.

A B, Fig. 1. is a square trough, closed at the two opposite ends C D, by well polished plain glasses. It will hold any liquid through which the sun's rays are to be transmitted. E is a small spout, and F a handle; so that any portion of the liquid may be conveniently poured out, when the rest is to be diluted.

The trough is made to fit into the open part of the skeleton eye tube, Fig. 2. resting on the bottom G, and being held in its proper situation by the sides H and I, the end K at the time of observation is put into a short tube fixed to the Newtonian telescope, and may be turned about so as always to have the open part H I horizontal.

When the eye-piece Fig. 3. is screwed by its end M into the skeleton tube at L, Fig. 2. and the trough, Fig. 1. with any liquid to be tried is placed in the open part G H I, the sun's rays will come from the small mirror of the telescope to K, and passing through the plain glasses C D, inclosing the liquid, will enter the eye-piece M, and after the necessary refractions come to the eye at N. Any other single or double eye-piece of different magnifying powers may be screwed into L, instead of the piece Fig. 3; and the liquid may easily be tempered so as to intercept a proper quantity of light to suit every eye-glass which is in use, and thus to render the inspection of the sun perfectly convenient.

* Philof. Transf. 1801, p. 362, mentioned at page 21 of our Journal, but in that Number there was no room for the Plate.

XV.

On the Effects of the Respiration of the Nitrous Oxide; particularly an Instance in which the Excitation of the System produced unpleasant Symptoms. In a Letter from Mr. JAMES STODART.

TO MR. NICHOLSON.

S I R,

THE first opportunity I had of breathing nitrous oxide was in the month of June last, in company with Mr. Davy, in the laboratory of the Royal Institution. My curiosity had previously been much excited by what I had read and heard on the subject. I began breathing rather sceptically, but my doubts were soon removed. A slight degree of giddiness was followed by a pleasing thrilling and sense of warmth over the body; but not being used to breathe through a tube, and at the same time a little alarmed, I suffered a small quantity of common air to pass into the lungs, and by thus diluting the gas its powerful action was suspended. This first attempt increased my desire to breathe it in a pure state; and a few days afterwards I had another opportunity, in the company of the same gentleman. After exhausting the lungs by a forced expiration, and compressing the nostrils, I began breathing as before. The air tasted distinctly sweet; the slight giddiness was, as before, quickly succeeded by sensations very difficult to be described. I nearly lost sight of all sensible objects, and felt as if in a most delightful dream. On recovering from this trance-like state, my first feeling was certainly a sentiment of pride or disdain.—I could with difficulty descend to notice the objects around me. This emotion was succeeded by an irresistible propensity to muscular action, in which I indulged myself, by walking or rather stamping about the laboratory. It was in the morning that this experiment was made, and during the remainder of the day I felt highly exhilarated. In the evening I again breathed a quantity of the gas with nearly the same consequences. I found it impossible to resist the strong propensity to active exertions. At night I went to bed as usual, and after sound sleep I awoke in the morning more than usually cheerful.

Effects of breathing the nitrous oxide.

Taste of the gas. Giddiness.

Delirium.

Recovery.

Irresistible propensity to muscular action.

No depression followed.

Other trials.

Pain in the face
mitigated and
removed.

Striking phenomena of
deranged percep-
tions. Numbers
of people—mus-
ical sounds—
illumination of
coloured light.

Gas breathed at
the Royal Insti-
tution.

Usual effects.
Alarming conse-
quences next
day.

I have lately prepared and breathed the gas at different times, and the effects were always pleasurable. One circumstance I wish to notice. A pain in the right side of my face and head, which for some weeks had been very troublesome, was certainly at first increased, after breathing four quarts of the gas. The pain, however, gradually lessened, and in about an hour was quite gone, nor had I any return of it either during the remainder of the day or following night. From this I began to suspect that the gas had been instrumental in some manner in removing the pain. Some symptoms of its return during the following day induced me to give this new agent a further trial. With this view I prepared six quarts of the gas, and an hour before my bed-time I breathed it. I was soon wholly under its influence—totally lost to all sensible objects—all was visionary. I fancied a number of people in the room; imagined musical sounds; and the apartment appeared illuminated with varied coloured lights. A friend who was with me thinks I remained in this state a full minute after dropping the breathing-bag. My first enquiries were wild and incoherent; but I gradually recovered my usual temper of feeling, slept well during the night, and awoke free from pain in the face. I have felt very little of it ever since. Whether the nitrous oxide was the removing cause or not, I shall not take upon me to determine. I used no other means; nor do I know whether any other person has breathed the gas under similar circumstances. A better acquaintance with this most extraordinary agent would probably lead to important and useful discoveries.

Since writing the above, I have again breathed the nitrous oxide in a very pure state at the Royal Institution, and was as usual lost in pleasure. On recovering, I signified a wish to know the state of my pulse, and was told it was above 140. This was on Saturday the 13th, about three o'clock. I continued under the strong influence of the gas as to muscular action, &c. during the remainder of the day; but awoke early next morning with feelings very different to those I had formerly experienced. I felt a tremor, soon became faint, and this faintness increased so much, that for a short space of time I was as if sinking into nothing. I was certainly under considerable alarm. A mouthful of water recovered me from the apprehensions of fainting. For some hours afterwards I remained

remained in a very low state, during which I am certain that I repeatedly tasted the gas. But my mind was now perfectly tranquil, and at times I had feelings analogous to those I have sometimes experienced during the time of breathing the gas itself. During the whole of this and following day I felt depressed, with occasional slight giddiness; and as these feelings still continued, on the Tuesday morning I consulted Dr. Garnet, who considering the excitability of the system to have been exhausted by too powerful a stimulus, directed a cordial medicine and a moderate quantity of wine, the good effects of which I soon felt. It required a few days, with nourishing diet, to recover my former strength and spirits. I never intend again to breathe the gas in so large a quantity. I remain much at a loss to account for the effects produced by the last dose, so contrary to all my former experience; more especially as Mr. Davy does not think the dose was at all extraordinary in quantity, and the gas was not different from that usually prepared. It has stood over water for a day, and was breathed at the same time by Mr. Davy and another gentleman with no unusual consequence.

Extreme depression and occasional giddiness.

Medical treatment.

I am, dear SIR,

With much respect,

Your obedient Servant,

JAMES STODART.

Strand, Feb. 19, 1802.

P. S. I prepare the gas from the nitrate of ammonia; am particularly careful not much to exceed 400 degrees of heat, and have always suffered it to stand some hours over water before it is used. I procure the salt in a very pure state from Mr. Accum.

XVI.

*Remarks on the Processes for clarifying Liquids. By CITIZEN
PARMENTIER*.*

THE name of clarification is commonly given to a process by means of which liquids are deprived of such foreign substances as affect their transparency.

Clarifying is a
process of some
importance.

This operation, however simple it may be in appearance, deserves nevertheless to be particularly attended to, especially when we consider the advantages it affords in the chemical and pharmaceutic arts. From these motives, I have thought it might be useful to communicate some general notions respecting the process.

Various methods.

It is not my intention to develop in this place the different methods made use of to obtain this purpose, nor to shew at length the effects they produce with regard to each of the objects submitted to the operation. It will be sufficient simply to point out the principal phenomena attending operations of this kind.

Object of the
process.

The purpose to be answered, is, to clear the fluid from such bodies as, without being dissolved, remain suspended, and impair its transparence and limpidity: but these bodies sometimes are separated by repose or filtration; in other instances by the action of the air, of heat, of light, of motion, and of fermentation; and again in other instances by means of agents, which, by uniting the scattered particles in the liquid intended to be clarified, frequently change its nature, and no longer allow them to remain in their former state. We will begin our examination by attending to spontaneous clarification.

Spontaneous
clarifying of
fluids

This takes place only when the particles to be separated are decidedly of a less or greater specific gravity than that of the fluid in which they are suspended. In this case they may unite at the bottom of the fluid, or at its surface, and form a magma, which is very easily removed if the separation has been complete. After this the fluid possesses all the transparence which can be desired, and the most accurate filtration will add nothing to its clearness.

* Annales de Chimie.

This method of clarifying is sometimes subject to inconveniences, of which the principal are, that it requires much time, and tends by this delay to favour the formation of new products, which, by changing the composition of the fluid itself, no longer presents it, independent of the abstraction of the bodies which affected its clearness, the same as it was before its clarification. We find a very striking example of this kind when we consider what happens in the spontaneous clarification of the juices of plants or fruits. These juices when newly expressed are always turbid; they nevertheless become gradually clear; but their nature is not then entirely the same: they contain products which would not have been found if they had been clarified immediately after the expression. For this reason also it is that the juice of lemons, of gooseberries, &c. when examined before or after their spontaneous clarification, are so different in their taste, their colour, and their domestic utility.

affords time for other changes to take place;

In general we may consider it as a certain fact, that all the fermentable fluids are liable by spontaneous clarification to the effects here stated, whereas they do not take place with respect to such liquids as are little or not at all capable of fermentation, and of which the transparence is affected only by the interposition of particles incapable of acting in any manner upon the constituent parts of the fluids themselves.

and is not therefore suited to fermentable fluids.

Thus for example: Water, alcohol, ether, oil, &c. which in their first state may not have been perfectly transparent, may easily become so by spontaneous clarification, without the least change ensuing in their composition; for when we examine them after clarification, we find them in the same state as other similar fluids which have not been subjected to that process.

But other fluids are well adapted to this treatment.

The second process for clarifying fluids consists in filtering them; but this operation can never be performed without the assistance of intermediate bodies, of which the very contracted pores admit only of the passage of the fluid, and retain all the particles which were before suspended in it.

Filtration.

The instruments of filtration are exceedingly various. Paper, woollen cloths, linens, cottons, carded cotton, sponge, sand, earth, pounded glass, charcoal, porous stone, &c. all these bodies may be usefully employed in the present operation;

Different kinds of filters

tion;

tion; but their nature and their purity must first be examined more particularly when the fluid to be filtered is of a saline nature.

require selection. It is incumbent on the chemist to select from among the different filters, that which, by producing the clarification of the fluid in the best manner, shall not at the same time effect any change in its constituent parts. Now the choice to be made in this respect must be determined from the knowledge we possess of the nature of the fluid, and that of the kind of filter proper to be employed.

Paper If the fluid be aqueous, vinous, alcoholic, or oily, paper may be used without inconvenience, provided it be of good quality. This last condition is absolutely indispensable, for without this the filtration would frequently prove defective.

acts mechanically. We know that paper is a *mechanical* texture of vegetable fibres, which have undergone different preparations. The particles of this fibre, by intertwining, leave pores, the tenuity of which is always governed by the state in which the paste or stuff existed at the moment when it was converted into paper. If this tenuity was considerable, the pores become speedily obstructed by the sediment deposited from the liquor under filtration, and at this period the filtration ceases. On the contrary, if the pores be very open, the filtration is made with rapidity in an incomplete manner, because at the same time that the fluid passes through the pores it carries along with it the most minute particles which are suspended, and there are only the coarser which remain on the surface of the filter.

The great art is therefore to choose such paper as has its pores of a requisite magnitude to admit the fluid intended to be filtered, without suffering any of the particles which produced the turbidness to pass through.

Different kinds of paper for filtering; Two sorts of paper are found in commerce, which nearly produce this effect; and though they are not always as perfect as might be desired, these have hitherto been preferred to every other. The one, which is of an imperfect white, is known in France particularly by the name of *papier Joseph*. The other is a kind of grey paper, less coarse than that which serves for wrapping up cheap goods. Neither of these have any size.

The

The white colour of the *papier Joseph* shews that it has been manufactured with a superior material to that used for the grey paper. The fluids which have passed through it are always very transparent, but it has the inconvenience of being easily torn, and its pores are soon obstructed, so that its filtration is not effected with dispatch.

The grey paper may be used for a long time also in producing clear fluids. But as the paste out of which it was fabricated was not as pure as that of the *papier Joseph*, it always communicates a disagreeable taste to the fluids, owing to the solution of the foreign matters it contains. On this account precisely it is that certain fluids, such as whey, wine, ratifia, and other liquids for beverage, when filtered through the grey paper, have always an odour and taste, which delicate organs soon recognize. And hence it is that among those fluids some of them are more susceptible of alteration than when they have been filtered through the *papier Joseph*.

The nature of the paper requires more particularly to be considered when saline solutions are to be filtered. If the grey paper be used, it frequently happens that part of its substance is dissolved by the saline liquor, so that it becomes less pure than before. This inconvenience, which is not so perceptible when the *papier Joseph* is used, may be still further diminished, by using the precaution of employing such filters only as have been previously washed by repeated filtration of boiling water. The exact pharmacist ought to be always provided with filters thus washed, in order to have recourse to them upon occasion. Josse, so distinguished in pharmacy, and to whom we are indebted for many important observations, has found the advantage of these instruments under a multitude of circumstances. He has, among other observations, remarked, that whey clarified and filtered through (*du papier raisin*) may be kept for more than fifteen days, by filtering it daily, which could not be done if it had been filtered through the common grey paper, even though previously washed.

By an effect directly contrary to this, different juices and plants are preserved transparent, and in good condition, without passing to the acid state, in consequence of their having been daily filtered through the grey paper. It was observed merely that their colour became more intense on the first days, and that they afterwards became insensibly discoloured.

But

Various precau-
tions.

But while we consider the nature of filters, their form and situation are no less important. In order that a filter of paper may produce its full effect, it must not adhere by its whole surface against that surface which supports it, for if this were the case the filtration would be soon stopped. This inconvenience is avoided by dividing it in different directions. But as the folds are soon flattened, some prefer placing between the support and the filter, straws or simple tubes of glass. I must confess that this last method has not always succeeded with me, and that I have most commonly observed, that the folds made in filters produce nearly as much effect as the pieces of straw and tubes. In Germany (and in England) they have funnels grooved or fluted within for this use.

Whatever may be the precautions which have been taken, a period will arrive at which the filtration becomes slow, and at length totally stops. This effect takes place when the pores of the paper are so much obstructed, that they no longer admit the passage of the fluid. Sometimes the filtration may be prolonged by giving a slight circular motion to the funnel; but this effect is of short duration, and there is no other remedy but to change the filter itself. It seems that there has not yet been found any means of remedying this inconvenience, which is common to all filters whatever.

Filters of wool-
len;

We have before observed that filters are made of woollen cloth, of piece goods, and of carded cotton. The woollen were formerly much used, and were even the first filters adopted. They were formed into the figure of a cone, of which the base was kept open by an hoop that was fixed in a frame with supporters. This kind of filter is still used to filter ratafias, as it may be rendered very capacious, and is susceptible of receiving a large quantity of fluid at once; but it affords little, and it is therefore necessary to wait a long time before the fluid passes clear; for which reason it is seldom used, excepting when no other apparatus can be procured.

and their uses.

Nevertheless, when the filtration of syrups is required, woollen cloths are used; but then instead of giving them the form of a cone, the operator simply fixes his cloth in a square frame, fastening the four corners upon pins disposed for that purpose. The boiling syrup is poured in the middle, where it always forms a kind of concavity, and frequently after a few minutes the liquid passes very clear.

This filter may be used for many other fluids, particularly those of an aqueous nature, which do not contain potash or soda in solution. For if they were only slightly alkaline, the filter would be soon destroyed, and the filtered liquor would not possess the requisite qualities.

Linen or cotton cloths, and paper, are commonly used for alkaline liquids, and they succeed very well, particularly when those fluids are not too concentrated. With regard to carded cotton, it is reserved to filter such fluids as are of considerable price, or very scarce.

Linen or cotton cloths, or paper, to what liquids applied.

This filter is made by introducing carded cotton into the tube of a glass funnel, where it is lightly pressed together with a glass rod so as to form a kind of stopper; after which the fluid intended to be filtered is poured into the funnel. The filtration takes place drop by drop and after the first drops are separated, those which follow are always clear. The essential oils may be very well filtered by this method, without any fear of that loss which would necessarily follow if the other filters before spoken of were used.

Carded cotton.

The acids, particularly those which are concentrated, can only be filtered through pounded glass; but care must be taken not to use this substance till after it has been washed several times, first in a large quantity of water, and afterwards in an acid, in order to deprive it of the earthy substance which the acids might dissolve.

Filter of pounded glass;

Glass filters may be very well constructed in a funnel. The great art in order that they may produce the effect, is to fix in the first place some fragments of glass in the tube, and afterwards to have others which are smaller, and this process must be continued, constantly diminishing the size of the fragments until the powder lies to the thickness of three or four inches, the last stratum of which must be very finely pulverised.

how constructed.

This kind of filter operates so well, that in less than an hour it is possible to filter through a funnel of middling size several kilograms of acid.

Sand is also very commonly employed to clarify water for domestic uses. Sandy springs are in fact true filters, of which the effect is more certain, in proportion as the layers of sand are so disposed, that the water upon them may be obliged to pass through the sand successively, and leave those bodies which injure its transparency.

Filtering by sand.

The

The art of constructing fountains or springs with sand is not yet carried to the degree of perfection it is capable of, and though it may seem to be an object of little importance, it well deserves to fix the attention of philosophers.

Observations.

Experience also shews, that filters or fountains of sand cannot be used with success but for a limited time. It is necessary to change the sand frequently, or at least to wash it, in order to deprive it of the earthy heterogeneous substance, deposited by the water, and which when accumulated to a certain point would not only oppose or diminish the filtration, but also communicate to the fluid a flavor, which is the more disagreeable in proportion to the length of time it remains in contact with it. Nothing is more easy, as is well known, than to deprive river water of the earth which it suspends, and which obscures its transparency. For this purpose it requires only to be left a few hours in an earthen vessel uncovered, because the action of the air is necessary to effect this precipitation speedily and completely.

(To be continued.)

SCIENTIFIC NEWS.

*Extract of a Letter from Brunn, in Moravia, dated
January 3, 1802.*

Tellurium is
asserted to be
antimony only.

A CHEMIST of Vienna pretends to have observed, that the new metal *tellurium*, discovered by Klaproth, and generally acknowledged as such, is nothing else but *regulus of antimony*: an observation which, according to my own experiments, has much probability.

Water said to be
decomposed by
magnetism.

Galvanism is at present a subject of occupation of all the German philosophers and chemists. Tromsdorff has burned various metals by means of a pile of 150 plates; and at Vienna a discovery has been made, that an *artificial magnet*, employed instead of a Volta's pile, decomposes water equally well as that pile and the electrical machine; whence (as they write) the *electric fluid*, the *galvanic fluid*, and the *magnetic fluid* are the same. *

* I applied the poles of a five bar magnet to two steel wires in the tube of water, having their extremities in the water less than one-tenth of an inch asunder, and perceived no effect.---W. N.

On

On the Muriatic Acid.

The reading of Mr. Chenevix's paper upon oxygenised and hyperoxygenised muriatic acid, before the Royal Society, was concluded on Thursday the eleventh.

Chenevix on the
states of the
muriatic acid.

After a short account of the experiments that had been made before him, and particularly of the ingenious conjecture of Mr. Berthollet,* he states the means by which he has ascertained, that the acid contained in hyperoxygenised muriate of potash is muriatic acid, in a particular state of combination with oxygen; and the experiments by which he determined the proportion of those elements. From the quantity of oxygen and of simple muriatic acid contained in the salt, he proves that hyperoxygenised muriatic acid consists of

Oxygen	65
Muriatic acid	35

100

From the proportions of the salt which is formed when a current of oxygenised muriatic acid is passed through a solution of potash, and which he found to be composed of the same elements, and in the same proportion as oxygenised muriate of potash would be, if at the very moment of its formation it had not been resolved into simple and hyperoxygenised muriate, he concludes that oxygenised muriatic acid is composed of

Oxygen	16
Muriatic acid	84

100

From a number of experiments, which are stated at length, Mr. Chenevix imagines that the salts of the genus oxygenised muriate, do virtually exist; but that by superior affinities they are resolved into muriate and hyperoxygenised muriate at the very moment that the acid comes into contact with the bases.

He describes and analyses the salts of the genus hyperoxygenised muriate; and mentions the hyperoxygenised muriate of ammonia as an extraordinary instance of disposing affinities.

* Journal de Physique, 1788, page 217.

Beneix on the
xtra of the
muriatic acid.

He obtained the alkaline salts pure by repeated crystallisations, and the earthy salts by boiling them with phosphate of silver.

He has examined likewise several of the metallic salts of this genus, and mentions hyperoxygenised muriate of silver, as strongly marking the difference between muriatic and hyperoxygenised muriatic acid. He ascribes to this salt, when mixed with combustible bodies, an expansive force, which he thinks he will be much within bounds, if he states to exceed five times that of any known detonating salt.

He concludes with an appeal to the chemical world, whether, in the present state of the science, it would not be more philosophical to say,

Muriatic radical, or some one word analogous to it, Muriatous acid, Muriatic acid,	Instead of	Muriatic acid. Oxygenised muriatic acid. Hyperoxygenised mu- riatic acid.
--	------------	---

and states the arguments in favour of either mode of appellation.

As a warning to those who would repeat his experiments he relates, in the course of his paper, an accident that happened in his laboratory to himself and to Mr. Vandier, by which the latter gentleman had almost lost his sight, and was wounded in the most dreadful manner.

A new Method of obtaining the Gallic Acid pure. By M. FIEDLER, Student in Pharmacy.*

As alumine has a great affinity with many vegetable substances, I inferred that this property might be applied to disengage the gallic acid from the extractive and the tannin which always adhere to it; and by that means to obtain the acid pure and easily crystallizable.

Solution of alum
precipitated by
potash.

Experiment 1.—I dissolved two ounces of alum in boiling water; I filtrated and precipitated it by a solution of potash; I separated the precipitate, and edulcorated it till the water of the lixiviation no longer rendered the muriate of barytes turbid.

* *Journal de Chimie, par Van Mons. I. 85.*

Experiment

Experiment 2.—I infused, to a reduction of one half, an ounce of good gall nuts in sixteen ounces of water. The result was a brown infusion, which after many filtrations continued turbid.

Nut-gall infused in water.

Experiment 3.—I mixed with the preceding infusion the precipitate of alumine, and frequently agitated the mixture with a glass rod. The next day I filtered, and observed that the matter passed perfectly clear, which convinced me that the alumine had precipitated the extractive. I washed the precipitate with warm water, until the water no longer blackened sulphate of iron, after which I left it to dry.

The precipitate of alum, being added to the infusion, combined with the extractive part.

Experiment 4.—As the alumine had evidently separated the extractive substance from the infusion of gall nuts, I could only suppose that the liquor of the filtration must contain tanin at the same time with the gallic acid. I ascertained the presence of the latter, by a solution of sulphate of iron; and to discover the former, I proceeded in the following manner.

The clear fluid might contain tanin and gallic acid.

Experiment 5.—I dissolved two scruples of isinglass in an ounce of water, and poured a few drops of this solution into the liquor of the gallic acid; they did not make it turbid in the least; which proved that it did not contain any tanin, which, with the gelatine, ought to have formed an elastic substance, insoluble in water. As the tanin was not discoverable in this liquor, there remained no doubt of its having combined with the alumine, which I ascertained in the following manner.

But, by the test of isinglass, it contained no tanin. Consequently the tanin was combined with the alumine of Exp. 3.

Experiment 6.—I digested a portion of the precipitate of the infusion of gall nuts with diluted sulphuric acid. It was entirely dissolved. The liquor passed clear through the filter, but coloured by the extractive matter. I dropped in a little of the isinglass solution, and immediately observed white filaments floating in the liquor, which by agitation formed into a mass, possessing all the properties of tanned glue; namely, of becoming elastic by heat, of softening by water, insolubility in alcohol, &c.

Precipitate of No. 3. was dissolved in sulphuric acid, which seized the alumine, while the tanin was precipitated by mucilage.

Experiment 7.—The filtered liquor, as well as the water of the lixiviation in the third experiment, of course held in solution nothing but the gallic acid. I concentrated the two fluids by slow evaporation, and obtained a salt in fine needles, which was the pure gallic acid.

The clear solution of gallic acid, Exp. 4. afforded fine crystals of gallic acid.

The

*The Seven following Articles are Extracts of a Letter of CIT. VAUQUELIN to CIT. VAN MONS *.*

1. Discovery of the Chromate of Iron in France.

Chromate of iron in France.

The chromate of iron has been found in abundance in the department of Var: we can now prepare any quantities of this metal, and study its properties in detail more than we have hitherto been able to do. Above all, we can compose in a direct way the chromate of lead, which is a very interesting object in painting. The chromate of iron may also be used to form a beautiful green on porcelain or artificial stones.

2. Discovery of the Emerald.

Emerald in France;

In a mineralogical tour which Cit. Lelievre, member of the Council of Mines has just made, he has discovered a quarry of emeralds, which are so abundant, that they are used in the country to pave the roads. They are found in the environs of Limoges mixed with granite, often without any regular form, and sometimes crystallized, but their colour and transparency are not beautiful.

3. Discovery of the Neutral Phosphate of Iron.

and neutral phosphate of iron.

Cit. Lelievre has likewise discovered in the same place a mineral sufficiently interesting, namely, a perfect phosphate of iron, that is to say, that the iron is entirely saturated by the phosphoric acid, a combination which had not hitherto been found in nature; it has a red brown colour, is semi-transparent, and has a foliated texture.

4. On the Zoonic Acid.

The supposed zoonic acid is the acetous;

Cit. Thenard has just finished an examination of the zoonic acid, in which he appears to have demonstrated that it is nothing but the acetous acid combined with a peculiar animal matter.

5. On the Cobaltic Acid.

and the cobaltic acid is arsenic.

Cit. Darracq has read a paper to the Institute, in which he shews that the acid found in zaffre by Cit. Brugnatelli, and which he named the cobaltic acid, is absolutely nothing but the arsenic acid.

You see that by degrees the number of chemical substances diminishes, which is a great advantage to the science.

* Journal de Chimie, par Van Mons. I. 218.

ACCOUNT

ACCOUNT OF BOOKS OF SCIENCE.

Elements of Chemistry. By J. Murray, Lecturer on Chemistry, &c. Edinburgh, 1801. Longman and Rees, London.

THE author of this work appears to be well acquainted with every thing at present known relative to the science of chemistry. He follows the classification of substances according to the order of their simplicity, and has given a digested, concise, and perspicuous detail of every important fact concerning their chemical relations.

Murray's elements of chemistry.

The view he takes of the present state of our knowledge respecting the important subject of heat is clear, candid, and satisfactory. On this, as well as on other controverted points, after stating the leading arguments on both sides he presents his own opinion where requisite, in the modest form of a query. On the whole, this work may be considered as a safe and clear guide to the student of chemistry; and as an useful course previous to the study of the more extensive system of Fourcroy.

B. B.

A Syllabus of a Course of Lectures on Natural and Experimental Philosophy. By Thomas Young, M.D. F.R.S. Professor of Natural Philosophy in the Royal Institution of Great Britain. Quarto, 193 pages. From the press of the Royal Institution, where the work is sold, and at Cadell's, in the Strand, London.

This syllabus appears to be one of the most complete and accurate outlines of science which has been hitherto published. It is divided into four parts, and each of these into subordinate sections, consisting of paragraphs regularly numbered. I cannot pretend in this notice to enter into any discussion of the numerous subjects which, in the nature of things, must be very concisely stated in such a work, and of which the illustrations must be sought in the lectures of the learned professor; but some intimation of the arrangement and objects of instruction will be gathered from the titles as follow. Part I. Mechanics. Of induction; motion; composition of motion; centre of inertia and of momentum; accelerating forces; central forces; projectiles; motion in given surfaces; collision

Young's syllabus of philosophical lectures.

Young's syllabus
of philosophical
lectures.

and energy; pressure; equilibrium; rotatory power; preponderance; practical mechanics in general; geometrical instruments; perspective; statics; friction; passive strength; architecture; carpentry; wheel-work; union by twisting; economy of motion; time-keepers; raising weights; friction wheels; carriages; compression and extension; penetration and attrition; trituration and demolition. Part II. Hydrodynamics. Of hydrostatic equilibrium; floating bodies; specific gravity; pneumatic equilibrium; hydraulics; resistance of fluids; hydraulic machines; pneumatic machines; sound; harmonics; properties of light; dioptrics and catoptrics; optical instruments; physical optics; nature of light. Part III. Physics. Of the fixed stars; the sun; primary planets; secondary planets; comets; laws of gravitation; sensible effects of the celestial motions; practical astronomy; geography; tides; general properties of matter; heat; electricity; magnetism; meteorology; natural history. Part IV. Mathematical Elements. Of quantity and number; space; the comparison of variable quantities; the properties of curves; practical rules and tables.

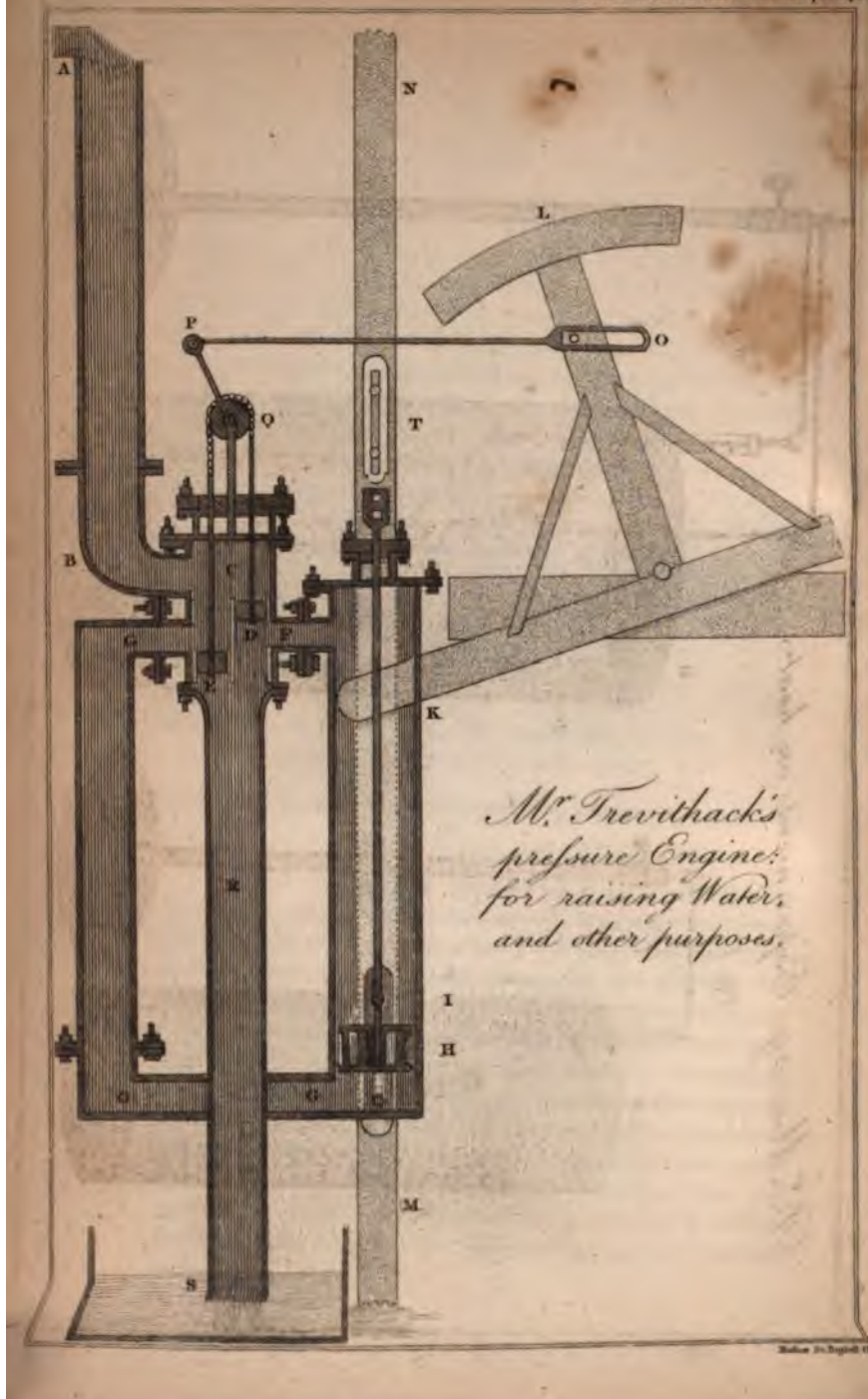
The whole work is beautifully printed, and such demonstrations as occur are distinguished from the narrative or enunciation by a smaller type. The figures are extremely neat. A fifth part is mentioned as probably hereafter to appear, containing a catalogue of the best authors.

MR. BLAIR'S POPULAR LECTURES.

Mr. Blair has recently commenced a course of popular lectures, at the Bloomsbury Dispensary, for the information of scientific persons, amateurs of natural history, and students in the liberal arts. He proposes to explain and illustrate the following subjects, on Tuesday evenings, at eight o'clock:

On the component parts of the body—the bones, cartilages, and ligaments—muscles, and muscular action—the integuments and membranes—brain, nerves, and sensation—the heart and vascular system—the blood, circulation, and absorption—glands, secretions, and excretions—respiration and animal heat—digestion, nutrition, and growth—utero-gestation, and parturition—the eye, and phenomena of vision—functions of the ear, nose, and mouth—physiognomy, beauty, and the passions.





Dr Murray's Experiments on heat.

Fig. 1.

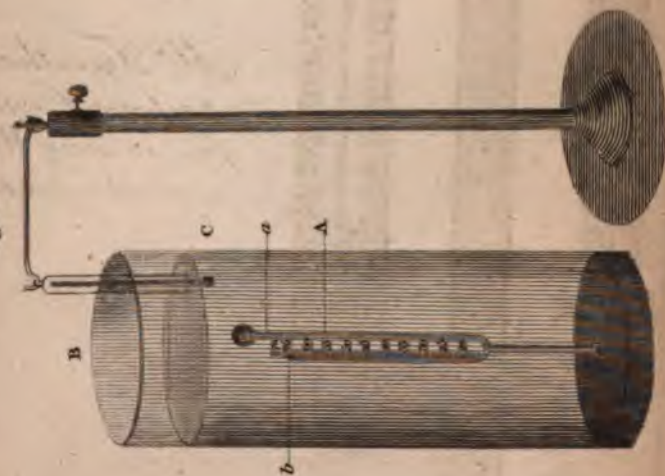
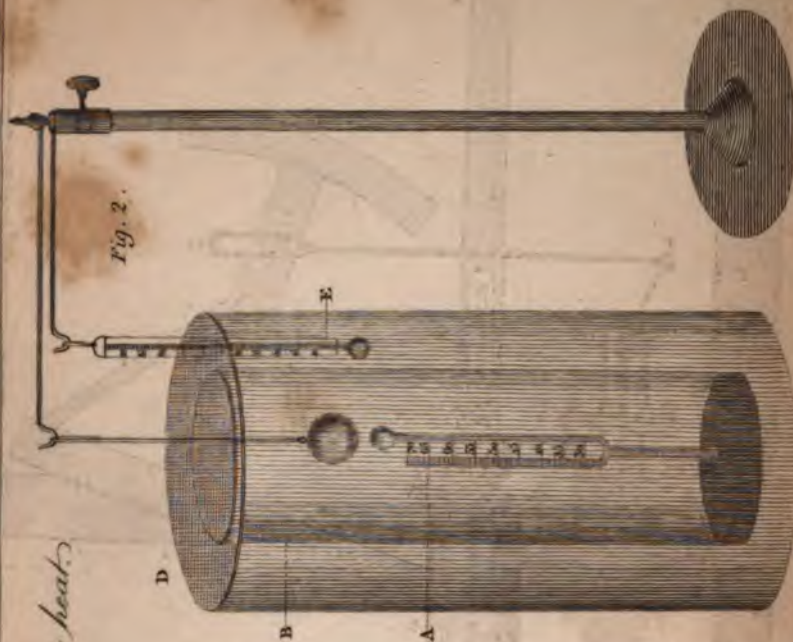


Fig. 2.





*Liquids substituted instead of
dark Glasses by Herschel.*

Fig. 1.

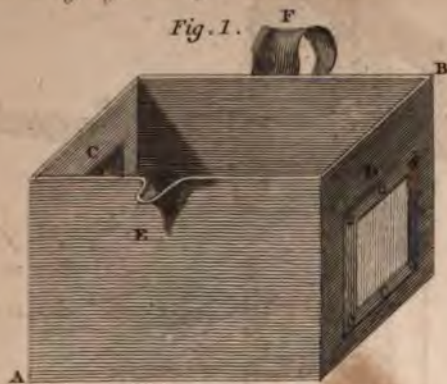


Fig. 2.

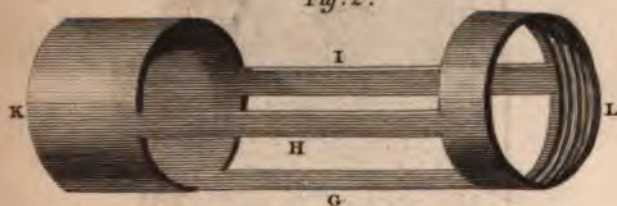
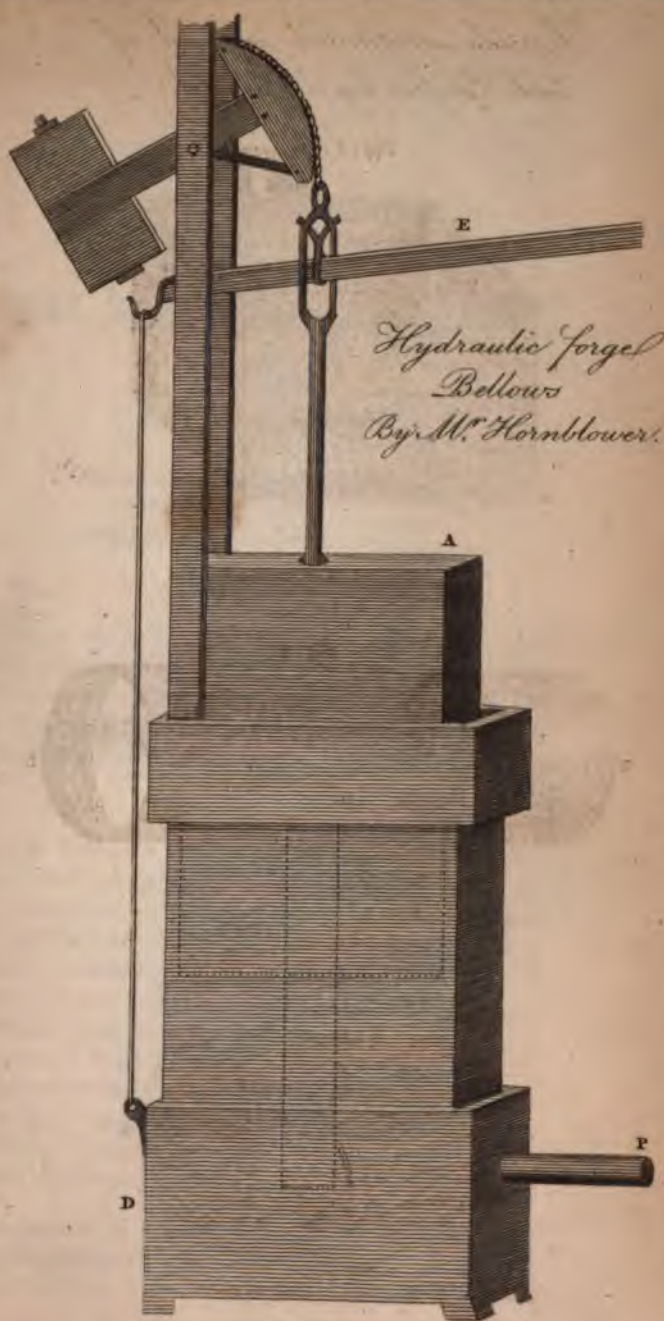


Fig. 3.







A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

APRIL, 1802.

ARTICLE I.

Experiments on the Transmission of Heat downwards through Mercury and through Oil contained in Vessels of Ice; by which those Fluids are proved to be proper Conductors of Heat. By Mr. JOHN MURRAY, Lecturer in Chemistry, Materia Medica, and Pharmacy, at Edinburgh. Communicated by the Author.

IN a former Memoir * I related several experiments made with the view of determining the question, whether fluids are capable of conducting caloric, and stated the sources of fallacy by which any strict conclusion from these is invalidated. The experiment calculated to determine this question with greatest certainty is that of heating a fluid downwards by bringing a hot body into contact with its upper surface; but in making this experiment a source of error is always present in the conducting power of the vessel in which the fluid is contained. When the upper portion of fluid has its temperature raised by the application of a hot body, part of it must flow towards the sides of the vessel, and give out part of its caloric; the solid

Review of the experiment of transmitting heat downwards through a fluid.

* Philof. Journal, I. 165. where by mistake the author's titles are erroneously given.--W. N.

matter of the vessel must convey more or less of this caloric downwards, and communicate it to the fluid beneath; and thus caloric may appear to be conducted to a considerable depth, though the fluid do not actually possess a conducting power. In any usual mode of performing this experiment, this source of fallacy cannot be intirely obviated; nor can its effects be estimated with such accuracy, as to determine precisely how far any observed rise of temperature in a fluid is to be ascribed to its operation.

Error from the vessel, avoided by forming it of ice.

It occurred to me, that this error might be completely avoided by employing a hollow cylinder of ice to contain the fluid. Suppose a thermometer to be placed horizontally within such a vessel, its bulb being in the axis of the cylinder, and covered with a fluid at the temperature of 32° . Let a hot body be brought in contact with the upper surface of the fluid, or even immersed in it, but still so as to be at some distance from the bulb of the thermometer; if the thermometer rise it may be concluded that the fluid has a conducting power, since the caloric could reach the bulb in no other mode. Caloric does not pass through fluids by radiation; it is easy to perform the experiment so as to prevent the thermometer from being heated by any motion of the fluid, from immersing the hot solid; and lastly, no caloric could be transmitted by the sides of the vessel, since ice cannot have its temperature raised above 32° , and cannot therefore communicate any temperature above that to a contained fluid. The conclusion therefore seems undeniable, that if in such an experiment the thermometer rise, caloric must be conveyed to it by the fluid interposed between it and the heated solid.

If the thermometer do rise the fluid is a conductor.

Whether the opposite conclusion may be affirmed, in case the thermometer do not rise?—No.

It seemed doubtful, however, whether the converse of the proposition would hold equally good. If the thermometer were not to rise, would this prove that the fluid is a perfect non-conductor? It is not evident *a priori*, whether a fluid contained in a vessel of ice is capable of being heated above 32° , or at least could convey caloric above that temperature to any perceptible distance, even allowing it to have a conducting power. Suppose that the heated solid is immersed in the fluid, the particles in contact with it will be heated, and will form an ascending current which must flow towards the sides of the vessel. The ice will abstract the excess of caloric from these heated particles, and they will return to the temperature

perature of 32° . It appears doubtful, therefore, whether any part of the fluid beneath the heated solid, supposing it to be capable of conducting caloric, can have its temperature in such a situation raised; and hence it appears uncertain, whether from the circumstance of the thermometer not rising in such an experiment, it could fairly be concluded, that the fluid is a perfect non-conductor of caloric.

There are also reasons, however, which render it probable, ^{if the heat be given out faster than the ice can absorb it, the thermometer will rise if the fluid be a proper conductor.} that any conducting power in the fluid if it do exist, may be discovered by an experiment of this kind, by the rise of the thermometer. Supposing the fluid to possess such a power, it is sufficiently possible that the caloric may be conveyed from the heated solid through the interposed fluid, (especially if the quantity of the fluid be small) to the thermometer, more quickly than it can be cooled by the ice. Two facts render probable this supposition. 1st. Ice is an imperfect conductor, and therefore absorbs caloric slowly from another body. If a piece of ice be thrown into water at 40 or 50° , a considerable time elapses before the temperature of the fluid is reduced to 32° ; and 2dly. The celerity with which a hot body gives its caloric to a cold one, is, *ceteris paribus*, proportioned to the difference between their temperatures. If the hot body, for example, have a temperature 100 degrees higher than the cold one, it will communicate caloric to it with much more celerity, than if it were at a temperature only five degrees higher. This cause operates in the present experiment. The difference between the temperature of the heated solid suspended over the thermometer, and that of the fluid interposed is very considerable, and it will therefore give caloric to it rapidly. On the other hand, the difference between the temperature of the particles of the fluid that are heated, and that of the ice with which they come in contact must be much less, and therefore they will part with their caloric more slowly. From this circumstance, independent of the slowness with which ice absorbs caloric, the communication of caloric in this experiment must be more rapid than its abstraction; and therefore if the fluid do possess a conducting power, the thermometer must be heated.

In reflecting on this subject it seemed even possible, that the fluid might be cooled so much more slowly by the sides of the vessel, than it was heated by the solid suspended in it, that the

Whether the stratum of heated fluid can extend below the hot heated body, and thus

raise the thermometer?—Not unless the vessel conducts; which in this case it does not.

heated particles might accumulate, and perhaps at length come in contact with the bulb of the thermometer, raise its temperature, and thus give the appearance of the fluid possessing a conducting power, though it had none. I was soon satisfied, however, that this could not be the case. Supposing the caloric to be ever so slowly abstracted from the hot particles, still the stratum of fluid directly heated, can never extend beneath the solid which is the source of the caloric. It is from it that the current of heated particles ascend, and they descend by the sides of the vessel, where their temperature may be reduced partially or intirely. But this descending current cannot proceed lower than the point where the ascending one commences, and of course no part of the fluid which has thus been directly heated can reach the bulb of the thermometer. In glass vessels this is not the case, because in these caloric is given out by the sides of the vessel, even beneath the heated solid, which is suspended within it. But in an ice vessel there is no communication of this kind, as it cannot conduct caloric at any temperature above 32° .

But if the thermometer rises, the fluid conducts; and if not, the fluid is probably a non-conductor.

The experiment then which has been stated is subject to none of these sources of fallacy. If the thermometer rise, there can be no doubt that the fluid conducts caloric. If it do not rise, it may be concluded, if not with equal certainty, at least with the greatest probability, that it has no conducting power.

A vessel was formed of ice.

A quantity of water was frozen in a tin mould, so as to form a hollow cylinder A, Pl. XIII. Fig. 2. the diameter of the cavity of which was three inches, and the depth $3\frac{1}{2}$ inches, the thickness of the ice forming its sides and bottom, being $1\frac{1}{2}$ inch. A thermometer B was introduced into it horizontally at the depth of one inch, the bulb being in the axis of the cylinder, and was frozen in, so that the vessel was capable of containing any fluid. It is evident that by pouring a fluid into this ice cylinder, so as to cover the bulb of the thermometer to a greater or less height, and by suspending in it a heated solid, any propagation of caloric through that part of the fluid beneath the solid, will be ascertained by the rise of the thermometer.

Water could not be used in these experiments, because it contracts by heat

For making this experiment with a view to determine the conducting power of fluids, water is altogether unfit, from the singular property it has of expanding instead of contracting in every reduction of temperature from 40° of Fahrenheit to 32° .

Suppose

Suppose the cylinder of ice to be filled with water at 32° , and a heated solid to be suspended in it, the portion of water in contact with the solid, will immediately have its temperature raised perhaps two or three degrees. But by this increase of temperature it is not expanded, but is contracted in volume, it becomes heavier, and must therefore sink in the fluid towards the bulb of the thermometer; it will be succeeded by another heated portion, a descending current must thus be formed, and the thermometer will have its temperature raised by the contact of particles which had been directly heated by the suspended solid.

near 32° , and would descend to the thermometer.

Oil and quicksilver are not liable to this source of fallacy. It has been ascertained by Crawford and De Luc, that the diminutions of volume in these fluids from decrements of temperature are nearly uniform, or at least at any temperature necessary for performing experiments of this kind they contract with sufficient regularity, as is indeed sufficiently shewn by their use as thermometrical fluids—With these fluids therefore the following experiments were made:

Oil and mercury do not contract by heat; at least about the temperature here mentioned.

Experiment I. A quantity of almond oil was poured into the ice vessel, so as to cover the bulb of the thermometer one quarter of an inch. A small iron cup C, cylindrical, two inches in diameter, and flat in the bottom, capable of holding two ounces by measure, was suspended so as merely to touch the surface of the oil, and was filled with boiling water. This was preferred to a solid ball, as presenting a larger surface to the oil, and as it was more easy to ascertain the precise depth at which it was immersed. At the beginning of the experiment the thermometer stood at 32° . In a minute and a half it had risen to $32\frac{1}{4}$, in three minutes to $34\frac{1}{2}$, in five minutes to $36\frac{1}{4}$, in seven minutes to $37\frac{1}{2}$. At this point it became stationary, having risen $5\frac{1}{2}$ degrees of Fahrenheit's scale in seven minutes. The temperature of the water in the cup had in this time fallen to 96° . The thermometer, after remaining stationary at $39\frac{1}{2}$ for six minutes began to fall, and it continued to descend at the rate nearly of a degree in a minute and a half, till it returned to 32 . At the conclusion of the experiment the sides of the cylinder at the upper part were partly excavated, the excavation was greatest at the surface of the oil, it became less as it descended, but it had taken place in a slight degree, even lower than the bulb of the thermometer.

Experiment I. Almond oil at 32° in the ice vessel was heated by water poured into a stationary metallic vessel slightly immersed in it. The heat descended to a thermometer.

Excavation of the ice.

meter, shewing that the fluid had been heated to this depth. The oil still covered the thermometer one quarter of an inch.

Exp. II. Repetition with a greater depth of oil.

Experiment II. The experiment was repeated, the oil covering the thermometer half an inch. The temperature was as formerly 32° , in three minutes the thermometer had risen to $32\frac{1}{4}$, in six minutes to $32\frac{1}{2}$, in eight minutes to 33 , in 12 , to 34 , in 15 minutes to $34\frac{1}{2}$, at which it became stationary. In this experiment, therefore, the thermometer had risen $2\frac{1}{2}$ degrees in 15 minutes: it descended as slowly as in the preceding experiment. In a third experiment in which the thermometer was covered three quarters of an inch with oil, it rose only $1\frac{1}{2}$ degree in the same time.

Exp. III. Repetition with mercury. Speedier transmission of heat.

The same experiment was next repeated with quicksilver in a similar cylinder of ice. 1st. The quicksilver covered the bulb of the thermometer one quarter of an inch. The small iron cup was not suspended, but allowed to float on its surface, and was filled with boiling water gently poured in. The thermometer began to rise instantly: in one minute it had risen from 32° to 36° , it remained at that temperature, or at least with the increase of half a degree, for another minute, and then began to fall; in three minutes it had fallen to 35 , the temperature of the water in the cup having fallen to 102 . The thermometer continued to descend slowly, till it returned to 32° .

Whether the subsidence of the mercury from fusion of the ice can affect the conclusion?

In this experiment there is a particular source of fallacy, against which it was found necessary to guard. From the melting of the sides of the cylinder of ice at the upper part by the contact of the heated fluid, the diameter of the cavity is enlarged, and therefore the column of mercury must diminish in height. In the experiment with the oil this does not take place, because the water formed from the melting of the ice being heavier, falls by the sides to the bottom, and supports the column of oil at its precise height, the one circumstance counterbalancing the other, since as much ice as is melted, as much water is produced, and very nearly the same volume is occupied by both. It was accordingly ascertained by exact measurement at the end of each experiment with the oil, that it had not sunk perceptibly; in other words, the bulb of the thermometer remained covered with the same height of fluid, as at the commencement of the experiment. But with the quicksilver the case is otherwise; the water produced by the melting

melting of the ice floats on its surface, and hence from the enlargement of the diameter of the vessel, the height of the column of mercury must be diminished. It was therefore necessary to ascertain with accuracy, whether this diminution had proceeded so far, as that the particles of mercury which had been in contact with the bottom of the iron cup, and directly heated by it, had come in contact with the bulb of the thermometer. This was found not to be the case. At the end of the experiment the bulb was still covered with so much mercury that the cup floated freely; and it was ascertained in particular both by actual measurement, and by a gage frozen in the side of the cylinder, that the quantity of mercury covering it was still one-eighth of an inch deep. As therefore the cup merely floated on the surface, the part of the fluid *directly* heated by it could not have come in contact with the bulb. This was likewise clearly established by the phenomena of the experiment itself; the thermometer rose rapidly at the commencement of it before the quicksilver could have sunk sensibly, and it remained stationary at the end when the full descent had taken place, although the water in the cup was still warm. On pushing it down so as to cause it to touch the bulb the thermometer rose rapidly, a proof that it had not previously been in contact with it. In this point of view therefore the experiment was unexceptionable, and those which follow were still less liable to any fallacy of this kind; as a larger quantity of fluid was interposed between the heated body and the thermometer.

Proofs that it
did not.

2d. As much mercury was poured into the vessel of ice as to cover the bulb of the thermometer half an inch, and water at 212° was poured into the iron cup floating on its surface. The thermometer stood at 32, in one minute it rose to $32\frac{1}{2}$, in two minutes to $33\frac{1}{2}$, in three minutes to $33\frac{1}{2}$, it then became stationary, and in six minutes more began to fall. The quicksilver in the vessel was found to have sunk rather less than one quarter of an inch. The thermometer therefore at the end of the experiment still remained covered with one quarter of an inch of fluid.

Exp. IV. Repetition with a greater depth of mercury.

3d. When the experiment was made a third time, the bulb of the thermometer being covered with one inch of quicksilver, the rise of temperature amounted to three-fourths of a degree.

Exp. V. and still greater.

Variation of
the circum-
stances.

All these experiments were frequently performed, they nearly agreed, and the above are the average results. Every precaution was taken to ensure accuracy, in which I was assisted by several friends. They were likewise occasionally varied. In one instance the cylinder of ice was made so wide, that a small thermometer was placed entirely within it in a horizontal position, and covered with oil, but the result was nearly the same. In others a solid brass ball was suspended in the fluid instead of the cup with hot water, but without any material difference in the result. In constructing the vessel it was found necessary that the ice should be frozen hard, that it might contain the fluid without allowing any of it to transude; and in the experiment with the mercury the external mould of tin was allowed to remain round the ice cylinder to give it strength, and enable it to contain the mercury without any risk. This however could make no difference in the experiment, and the result was found to be the same when the tin was removed.

Development of
the process.

In all these experiments then, a rise of temperature in the thermometer took place, greater or less, proportioned to the quantity of fluid interposed between it and the heated body. From the nature of the experiment this rise could not be expected to be considerable, since the fluid when heated must quickly have had its caloric abstracted by the cylinder of ice in which it was contained. Hence at a certain stage of the experiment the thermometer became stationary, though the heated body suspended over it still retained a considerably high temperature, the communication of caloric to the fluid not being equal to its abstraction by the ice; and when the communication became less rapid than the abstraction, the thermometer began to descend. In all these experiments, however, even in those in which the largest quantity of fluid was interposed, the rise of the thermometer was unequivocal, and in some of them, considering the circumstances of the experiment, very considerable.

Conclusion. *The
fluids were direct
conductors of heat.*

This rise it appears to me impossible to ascribe to any other cause, than to a power in the fluid to conduct caloric. Any other that might be imagined can be proved not to exist.

For the sides of
the vessel could
not conduct:

Thus it is evident, that the sides of the vessel could not convey to the fluid in contact with the bulb of the thermometer, any part of the caloric it received. Ice in common
with

with any other solid may at temperatures below its melting point conduct caloric; but as it cannot possibly exist with a temperature above 32° , it cannot communicate any temperature above that to a fluid in contact with it, and consequently it could not contribute in the above experiments to raise the thermometer above that temperature.

The experiment was performed in such a manner that no motion was occasioned in the fluid capable of conveying any part of it directly heated to the bulb of the thermometer; and indeed the regular, and in some of the experiments, the slow rise of the thermometer is inconsistent with the supposition of caloric being communicated by any cause of this kind. neither was the fluid agitated;

It has been proved, that any current produced in the fluid by the contact of the heated solid could not be the mean of conveying caloric; for if the fluid be supposed to have no conducting power, since the sides of the vessel are likewise incapable of communicating any increase of temperature, the portion of fluid beneath the surface could not possibly be disturbed by a heated solid merely resting on that surface. Even if the solid were immersed to some depth in the fluid, it is only from the sides and under part of it that a current could rise, and of course no part beneath that level could be affected. nor could any heated current descend;

It has likewise been shewn, that the rise of the thermometer cannot be ascribed to the contact of particles directly heated by the solid, and reaching the bulb from the sinking of the column of fluid by the enlargement of the diameter of the vessel from the melting of the ice; 1st. because in the experiment with the oil this sinking did not take place, the water produced supporting the oil at the same height; and 2dly. because even in the experiments with the mercury, the diminution did not take place to that extent which would have been necessary to have brought the particles which were heated on the surface of the fluid into contact with the thermometer, as was proved by actual measurement, and by the phenomena of the experiment itself. nor did the mass of warmed fluid subside to the thermometer;

Lastly, the caloric which reached the bulb of the thermometer could not have been propagated by radiation, because caloric, it has been proved, does not radiate through transparent fluids; and it cannot even be supposed capable of passing by radiation through an opaque fluid as mercury. The last point is obvious, and is of itself decisive with respect to this objection. nor was the heat transmitted by radiation.

Experiments of
radiation; with
remarks.

objection. But even the first, that caloric does not radiate through a transparent fluid, has been sufficiently proved by the experiments of C. Rumford. I shall add one of a similar kind made with the particular apparatus used in the preceding experiments, which appears to me sufficiently decisive. The thermometer in the ice cylinder was covered with oil one quarter of an inch, and the cup was suspended over it in such a manner that the bottom of it, though near to the surface of the oil, *did not touch* it. Boiling water was poured in, the thermometer rose scarcely one degree in five minutes, and did not rise more in any longer time. It has been shewn that when the thermometer was covered with one quarter of an inch of oil, and the cup filled with warm water, was suspended so as to *touch* the surface, it rose in seven minutes $5\frac{1}{2}$ degrees. Had this rise been owing to the communication of caloric by radiation, the variation in the experiment which has just been related, could not have produced any change in the result. The communication of temperature by the *conducting power* of the fluid, must be facilitated by the contact of the fluid with the heated solid, but the communication of it by *radiation* required no such aid, nor could it be promoted by it. The mere circumstance of the heated solid not touching the fluid, could make no difference with respect to the one, but must make the most essential difference with regard to the other, and the experiment clearly proves, that even through transparent fluids caloric does not radiate. The slight rise of temperature that took place, is owing to the caloric passing partly by *radiation*, and partly by communication by the medium of the air to the *surface* of the fluid, and from that it is conveyed by the conducting power of the fluid to the thermometer. The proof of this is, that even with mercury, a slight rise of temperature was indicated when the experiment was performed in the same manner. When the cup with hot water was suspended over the mercury covering the bulb one quarter of an inch, but not touching it, the thermometer rose one degree.

Concluding re-
marks.

It may therefore from the whole of these experiments be concluded, that the observed rise of temperature was owing to the fluid conducting the caloric, since no other cause can fairly be assigned. I have not subjected any other fluid to experiment, because it was difficult to find any of a nature sufficiently different, which the ice would have been capable of

of containing. But if it be proved that oil and mercury are capable of conducting caloric, it will be admitted as sufficiently probable that other fluids must have a similar power. Of these two, mercury it is probable, from these experiments, is the best conductor, as the rise of the thermometer took place in it much more rapidly than in the oil. That the temperature did not rise higher in it than in the oil, seems to have been owing partly to its greater mobility, by which its parts would move towards and from the ice with more celerity, and partly to its better conducting power, by which it would give out its caloric more quickly to the large superficies of ice with which it was surrounded. It was accordingly observed, that the sides of the ice cylinder were more excavated in the experiments with the mercury than in those with the oil.

Edinburgh, Feb. 27, 1802.

II.

*Description of a new Escapement for Watches, invented by Mr. JOHN DE LAFONS *.*

THE inventor states, that since the perfection of chronometers consists more in the equality of impulse given to the balance than to any other cause, he has contrived the present escapement for giving such an impulse; with the additional qualities of locking the wheels without spring work perfectly safe, and unlocking them with less consumption of power than in any other escapement he knows, because the wheels do not bear against the locking with more than one tenth part of the whole pressure from the main spring, which last circumstance he conceives to be perfectly new.

The perfection of chronometers depends chiefly on the equality of the impulses given to the balance.

He remarks, that the equality of impulse has been effected with great ingenuity by Mr. Mudge and Mr. Haley; but at the same time he objects to the escapement of the former, on account of its extreme difficulty and expence, and to that of the latter, for its very compound locking. And he also re-

Escapements of Mudge and Haley.

* From the Transactions of the Society of Arts. 1801. A premium of thirty guineas was given by the Society to the inventor: I have not copied the letter press, as I wished to render the description rather more minute than was necessary in that place.—N.

Description of a
new escapement.
Its parts.

plies to some observations which were made at the committee of the Society, which do not require to be stated here.

Plate XIV. exhibits the escapement, where the same letters denote the same things in all the figures. A is the scape-wheel, B a pallet lever on an arbor with fine pivots, having at its lower end, Fig. 1 and 2, a remontoire, or spiral spring, C, fixed with a collar and stud as pendulum springs usually are. H H represents the balance, having upon its verge K K, a pallet D, consisting of two arms, which carry a small roller moveable on fine pivots; and also certain other pallets E, represented more fully in Fig. 4, the plan of which parts is seen in Fig. 1, at E and D. F is an arm which serves to move two locking pallets *a* and *b*. It is continued beyond the center of motion, in order that it may balance itself in all positions, and at the part most remote from the balance there are a couple of studs and screws to adjust and bank the quantity of motion. The last mentioned pallets have their faces, portions of circles, and they move on an arbor with fine pivots, as may be seen at Fig. 2 and 4, in which figure also it is to be observed, that the arm of the locking pallets is formed into a triple fork, the branches of which are exposed to the action of two pallets E before mentioned.

Performance of
the escapement.

1. The scape wheel winds up a lever and spring; and is caught by a detent. But the re-action of the spring lever renders the hold of the detent exceedingly light.
2. The balance unlocks the wheel which lets go the lever, and this strikes a pallet on the balance arbor.
3. In its return the balance again unlocks the wheel, which again winds up the lever, (No. 1, 2, 3, &c.)

From the above description, and the consideration of the figures it will be apprehended, that, supposing the maintaining power to urge the wheel A forward, and the balance to be set in motion by external means, the lever pallet B will be wound up to its position in Fig. 1. by the tooth 1 of the wheel, at which instant the tooth 3 rests upon the pallet *a*. Suppose the balance to move in the direction of the small arrow near D, the verge pallet D will just pass the end of the lever pallet B, at which moment one of the two unlocking pallets E will carry before it the arm F, and unlock the pallet *a*, and consequently the wheel will drop with its tooth 4 upon *b*, and the lever pallet B will be set at liberty by the advance of the tooth 1, to act upon the balance by the pallet D, until B is stopped against the tooth 2, as seen in Fig. 5. By this means the balance receives the same invariable impulse at every second vibration. In its return the opposite unlocking pallet takes the other external arm of the fork of the locking pallets, and by that means unlocks *b*. The wheel immediately drops forward, almost through the space of one tooth,

tooth, so that the tooth 1, Fig. 5, falls upon the pallet *a*, while the tooth 5 winds the lever pallet B again into the position exhibited in Fig. 1. And the balance upon its return receives a second impulse under the circumstances already detailed.

It remains only to be shewn in what manner the middle prong of the fork is employed. Its use is to secure the locking of the pallets *a* and *b*. When the unlocking is performed by the action of either of the pallets *m m*, Fig. 4, the middle prong falls into the small notch in the roller *n*, which lies between those pallets; and when the unlocking is performed, and either *a* or *b* has received its tooth, the arm F renders it impossible it should be accidentally disengaged, because that arm is prevented from returning by the circular face of *n*, past which it could not return at all, if the notch or excavation were not made to permit to pass at the proper time.

Lastly, as the pallet lever B always rests against one of the teeth of the wheel A, the pressure against the pallets *a* or *b* may be rendered extremely light, so as to deduct scarcely any thing from the momentum of the balance at unlocking.

Fig. 3. exhibits a simpler mode of construction, in which a spring is substituted instead of the pallet lever with its spiral. The inventor remarks, that he should prefer this construction for a clock, but that the disadvantage from the weight of the spring in different positions is obvious*.

The detents or locking pallets have no spring, and are very safe.

Light pressure on the detent.

Another construction.

III.

Remarks on the Processes for clarifying Liquids. By CITIZEN PARMENTIER.

(Continued from page 234.)

NEVERTHELESS, though the use of the filter for water intended to be drank is of high antiquity, we must confess that the fountains constructed for this purpose not only deprive them of the earth which rendered them muddy, but likewise

Filtered water is insipid, because deprived of gas.

* Some account of Time-pieces and Escapements are given in the Philosophical Journal, Quarto Series, I. 56, 429. II. 50. III. 187, 342, 416.

of a super-abundance of air with which they are sometimes impregnated, and gives them that lightness or briskness, which is found in the waters of the Seine in a greater degree than in any other known stream. The proof of this is, that by repeated filtrations we may render water flat, heavy and unwholesome.

Thus when the specific gravity of the water of the Seine is required to be determined, it is adviseable to take it from the river at the time when it is limpid, or leave it to clear by subsidence, and not to take that in preference which has been filtered; for though this operation renders waters clearer, it changes them remarkably, by depriving them, as has been observed, of the air which they contain in super-abundance.

I know a person whose palate was so accustomed to water, that he could distinguish by the taste water filtered through sand, from that which had not been so filtered. The latter appeared to him more sapid and lighter; which proves, no doubt, the privation of this air, a fact which may also be very easily ascertained under the receiver of the air-pump.

The filter cannot render salt water fresh.

Certain individuals, who are interested to maintain the contrary to what is here asserted, have affirmed, that if water were continually forced to pass through 18 feet of sand or gravel upwards, it would be purified not only of heterogeneous matters, but completely purified that is to say of its salts. This prejudice was such, that in order to support the notion the following reasoning was offered:

If these filters be sufficient to deprive water of its air, why should not the same operation be equally proper to deprive it of the salts with which it is charged; but no attention was paid to the circumstance that these salts held in solution in the water, being specifically heavier, pass along with it through the smallest apertures, whereas air being specifically lighter, and existing in a different state from that of the salts, is easily separated. And this process of application was proposed to be applied to sea water in order to render it potable. The operation has even been announced to the government as new and ingenious. It consisted in a filtration effected by force in a direction contrary to that of gravity.

But the union of saline matters with water has not a purely mechanical division. They are not interposed as some philosophers have pretended, but perfectly dissolved in water, and possess

possess the same fluidity. These salts consequently become capable of passing through the finest filters. No other means but evaporation is therefore adopted to separate water from the saline substances which it holds in solution, and consequently distillation alone can produce this desirable effect. But to return to the general effects of filtration:

Independently of the filters here mentioned, water is also clarified by means of the stones called filtering stones. Of these there are several kinds. They are very porous, because sand enters into the greatest part of their composition. These stones being excavated are filled with water. The fluid gradually insinuates itself through their pores, and appears on the external surface in the form of drops of considerable clearness, which fall into a vessel placed beneath.

The filtering stone;

It is necessary that these stones should be previously washed by filtration of a large quantity of water. It is observed, that even for several days the water acquires a disagreeable taste, owing to the foreign matters dissolved by this fluid in its passage through the stone. Water thus clarified cannot be fit for use until it passes through intirely deprived of taste.

In general the filtering stone, though highly extolled, is a bad instrument to procure good water; for the filtration is made very slowly, and very often stops altogether, if the inner and exterior surfaces of the stone be not rubbed from time to time with a coarse brush, to detach the earth which the water deposits. To these inconveniences no doubt it is, that we ought principally to attribute the disuse into which this kind of filtration is fallen.

It is a bad contrivance.

It now only remains, that we should speak of the other processes which are used to give that perfect limpidity to several fluids, which they can never acquire by spontaneous clarification or by filters, whatever may be their structure.

Clarifying without the filter.

Though it is true, that the remarkable opacity of some fluids is owing to the interposition of particles not dissolved, but merely suspended in consequence of their extreme division; it is also certain, that under other circumstances the defect of transparency depends intirely upon the incomplete solubility of one or more bodies contained in these fluids, so that in order to give them the desired limpidity, we must necessarily have recourse to the methods which increase the solubility of the substances in question, or effect its total separation.

Opacity from imperfect solution;

Albumen

removed by certain additions.

Albumen, gelatine, the acids, certain salts, lime, blood, and alcohol, may in many cases concur to operate the clarification of certain fluids, for which the common filters would be insufficient. Nevertheless, these agents cannot be indifferently used, and the preference given to one rather than the other always requires to be determined from a knowledge of the fluid to be clarified. Accident has shewn for example, that two handfuls of marle reduced to coarse powder, and thrown into the fruit in the press, clarifies cyder and small cyder.

Albumen and gelatine used in fining wines, &c.

The effect of the albumen and gelatine is principally seen in vinous liquors. For this reason also they are used in fining wine, that is to say, in producing that bright clearness which they can seldom acquire, and preserve by simple repose. In this case it is sufficient, that one or the other of these two substances should be dissolved in a small quantity of water, and the solution mixed with the cold wine. A short time afterwards a kind of net-work is seen to form itself through the whole fluid, and soon afterwards this net-work contracting in all directions, collects all the foreign substances, and carries them to the bottom.

In some instances an acid is added.

In other instances it is necessary to heat the fluids with which the albumen has been mixed, and in these cases the clarifying is effected at the instant the mixture begins to boil. Most syrups are clarified by this process, and hitherto no other process has been discovered to produce a better effect. It has also been observed that albumen alone is not always sufficient to clarify liquids, even though they be heated sufficient to cause them to boil; but that it is necessary to assist its action by means of an acid, or salt with excess of acid. In proof of this we may offer the clarification of whey as an example.

In fact, it is proved that whey, in which albumen has been mixed, does not admit of the coagulation which carries the cheesy matter along with it, unless a portion of acidulous tartarite of potash or vinegar be added at the instant the boiling begins.

It may also be conceived that the quantity of acid requisite to be added in this case always bears relation to the state of the fluid, and that it would be absurd to pretend to fix the dose in an invariable manner.

Fresh

Fresh cream is advantageously used to clarify spirituous liquors. One or two spoonfulls for each French pint or English quart are sufficient to produce this effect, without heat, in the course of a few hours. But as in this clarification there always remains some cheesy particles suspended in the fluid, on account of their great subtilty, it is necessary to complete the process by subsequent filtration.

Cream used to clarify spirits.

Lastly, there are fluids which, in order to become clear, require only to be heated nearly to the boiling point. These are principally such as owe their opacity to substances which cannot be completely dissolved, unless the temperature of the solvent be raised considerably above the natural state. Many saline solutions are in this case, and those who are busied in chemical researches have frequent opportunities of observing them.

Mere heat.

Most of the juices of plants newly expressed may be partly clarified by heat. The chemist is therefore in the habit of recurring to this method with respect to the juices which, on account of their density and viscosity, are not capable of being filtered.

Clarifies the juices of plants, &c.

It frequently happens that a very slight degree of heat, applied to expressed and filtered juices of certain plants, will render them at once turbid; in which case the flocculent matter that floats in the liquid settles at the bottom of the vessel. This substance was considered by Rouelle the younger as the vegeto animal matter of wheat; but I shewed, in 1772, that it is a substance similar to the white of egg; which proves that we were authorized at that period to reckon the albumen among the products of the vegetable kingdom.

Vegetable albumen precipitated.

I must insist on an important observation, that in general it is necessary to separate the magma which is formed in liquors clarified in albumen, particularly when in order to concentrate these fluids it is necessary to evaporate them by boiling. Without this precaution we shall see the same magma dissolve, and the fluids become more turbid than they were before the clarification. For a like reason it is that soups, which have not been skimmed in time, always retain a cloudy and unpleasant appearance.

Caution against re-dissolution of the albumen.

Though the use of albumen to clarify the juices of certain vegetables is considerable, it is not, however, exempt from inconvenience. One of these inconveniences is, that it

In what cases fining with albumen is hurtful.

changes the nature of these fluids so much, that their medical properties are partly destroyed. What happens to certain pharmaceutical preparations, such as decoctions of medicines, when white of egg and heat is used to clarify them, is well known; for, in that case, they become almost ineffectual, if care has not been taken to double the proportion of ingredients which enter into their composition. Lewis has observed, that this operation deprives the syrup of diacodium of all its medical properties.

Such are the observations which I have thrown together respecting clarification. My design in communicating them has been to prove that this operation, though simple in appearance, cannot be practised without some skill and attention; and that among the number of processes commonly used, several afford results less satisfactory than others. It is necessary, therefore, to determine with regard to the choice of methods according to the nature of the substances made use of. I have confined myself to a clear narration of the facts; others may direct their attention to the explanations of the mode in which these substances operate.

IV.

On the Discovery of the Arseniates of Copper and of Iron. By a Correspondent.

Discovery of the arseniates of copper and iron attributed to Vauquelin. **A**N article * of the Decade Philosophique (30 Nivose), giving an account of the first trimestre of the tenth year of the French Republic, says, that Citizen Vauquelin has enriched the catalogue of mineral bodies by the discovery of two new substances, arseniate of copper and arseniate of iron. I am far from imputing to this able analyst the desire of attributing to himself any discovery to which he has no title; but the French chemists, whether from ignorance or intention, pay so little regard to the discoveries of others, that it becomes the duty of every friend of science to point out their violations of historical justice.

True statement of their history. Klaproth.

In the year 1786, Mr. Klaproth examined an ore from Cornwall, and found it to be arseniate of copper. Schlotheim published a description of it in 1790.

* By Cit. Lacede, Secretary.

In

In the first volume of the *Annales de Chimie*, page 195, ^{Proust.} there is an imperfect sketch of an analysis of arseniate of iron by Mr. Proust.

In February, 1801, the Count de Bournon presented to the Royal Society a very long and able account of a great number of varieties of these ores, which he had the good fortune to procure from Cornwall. This paper was followed by Mr. Chenevix's chemical analysis, which proved the accuracy of the Count's description. ^{The ample account of de Bournon, and Chenevix.}

The most extraordinary part of the paragraph in the French Journal is a confession, that the specimen examined by Cit. Vauquelin had been sent to the *Ecole des Mines* of Paris by the Count de Bournon. That gentleman never did send any until his account and Mr. Chenevix's had been made public. Cit. Vauquelin's paper must have been read at the National Institute between September 20 and December 20, 1801, six months at least after the communication to the Royal Society. Hence we see the justice of attributing that discovery to the French chemist. ^{Extraordinary circumstances attending this supposed mistake.}

In the *Journal de Physique* of Brumaire, tenth year, there is a paper of Dr. Karsten, of Berlin, upon the same subject: but which deserves no notice, further than as it regards the history of this substance. ^{Dr. Karsten's paper.}

I am, SIR,

Your humble Servant,

A CORRESPONDENT.

V.

*On the Nature and Preparation of drying Oils; with a View to the Improvement of such as are used by Artists, as Vehicles for painting.** By Mr. TIMOTHY SHELDRAKE.

EXPRESSED oils, considered with a view to the painter's use of them, may be divided into two kinds; first, such as are capable of drying in some circumstances by themselves, and always with certain additions; and secondly, such as cannot be made to dry by any means whatever. ^{Drying oils, and such as do not dry.}

* Transactions of the Society for the Encouragement of Arts, for 1801, page 209.

Drying oils:
linseed, nut, and
poppy.

Of the first, which I shall call drying oils, there are three in common use, linseed, nut, and poppy oil. The first is darkest coloured, and dries the soonest; the second is lighter, but does not dry so soon; and the third has least colour, and dries slower than either of the others.

Drying oils con-
tain mucilage.

By a process, which it is perhaps needless to describe here (*see Vol. xvii. page 281*),* I have succeeded in separating from each of those oils, a mucilage or gum, in a liquid state, and capable of mixing with water in every proportion, though, when thoroughly dry, it would not dissolve in cold water; but my experiments on this head were not carried to any great length. It is to be remarked that linseed oil afforded most of this gum, nut oil the next largest quantity, and poppy oil the least of all.

Olive oil con-
tains none:

Olive oil, when treated in the same manner, afforded none of this mucilaginous substance; whence I was led to conclude that the essential difference between the drying oils, and those which do not dry, consists in this:—that the latter either contains no mucilage or gum, or that it is so intimately combined with its other principles, that it cannot be separated from them in that peculiar manner which always takes place in oils which dry by themselves, or when mixed with colours.

Drying oil affords
a pellicle, which
dries first, &c.

If drying oil is exposed to the air, in a shallow vessel, and left at rest, a pellicle is soon formed on the top, and becomes externally perfectly dry. If this be removed, a second will be formed in the same manner; and if this experiment be repeated many times on the same quantity of oil, without moving or shaking the vessel, it will be found that the second pellicle will require more time to form it than the first, and so on, till it will be found difficult to get it fairly skinned over in a considerable time. The same effect takes place, in a less visible manner, in every quantity of drying oil which is united with colours in a picture.

From this experiment it is to be concluded, that drying oils exert that faculty by throwing up their mucilaginous parts, which become solid when at rest, and in contact with the air.

* Or *Philos. Journal*, quarto, III. 345.

The ingredients added to oils to make them dry faster, viz. calces of lead, saline substances, earths or gums, are such as unite with and increase the quantity of those parts which float to the top, and form a skin, more or less dark, over the colours originally mixed with them. If we consider the nature of these ingredients, we shall be at once enabled to account for a fact universally known, viz. that in proportion to the strength of the drying oil used in painting a picture its colour becomes depraved. It will be injured and finally destroyed, by being kept in a damp situation, excluded from a free circulation of air, or placed under a glass.

The desideratum is to prepare oil or other vehicle for painting, so that the colours, when mixed with it, shall not be debased under any of the above-mentioned circumstances. It must be so prepared or used, that it shall serve as a cement to unite and bind the colours, without skinning over them. It must likewise not contain those principles which always exist in the calces of lead, saline, or earthy substances, which from the first deprave the colours, and attract particles from the air, under peculiar circumstances, which increase that depravity, till at last the appearance of the colours is totally destroyed.

It is only among the resins or bitumens that we can expect to find a substance possessing the properties requisite to give to colours all the brilliancy and durability of which they are susceptible. My first attempts and experiments were made with solutions of mastic and sandarac in the painters oils; but though these compositions possessed more brilliancy than the common drying oils, they were liable to a considerable objection; for they did not dry readily, and when dry, were easily acted upon by all the common solvents for resinous substances, and on that account must be very deficient in durability, which is one of the most necessary qualities I wished to discover.

The difficulty with which amber is in any way dissolved, suggested the propriety of trying that substance. Accordingly I dissolved it, in each of the painters oils, by Dr. Lewis's process, without injuring its colour; and this solution was made in the common way. It was much darker coloured in itself, but produced scarcely any difference in effect when mixed with colour. By experiments with each of these solutions I ascertained the following facts, viz.

When oils are rendered siccativ they deprave the colours; and how.

Oils for painting ought not to skin; nor contain changeable ingredients.

Resins or bitumens answer this character;

Mastic and sandarac dry slowly, and are not durable.

Amber possesses superior qualities.

Every

Experiments.
Tints.

Every colour, and all the tints compounded from it, were more brilliant than corresponding tints and colours mixed with the best drying oils to be procured from the shops.

Paintings shut
up;

Colours mixed with amber, after having been shut up in a drawer for several years, lost nothing of their original brilliancy. The same colours tempered with oils, and excluded from the air, were so much altered, that they could scarcely be recognized.

colours exposed
to heat;

Colours tempered with amber were laid on plates of metal, and exposed (both in the air and close boxes) for a long time, to different degrees of heat, from that of the sun in summer to the strong heat of a stove, without being injured. It is needless to observe that oil colours cannot undergo the same trials without being destroyed.

and to solvents;

These colours, when perfectly dried in any way, were not acted upon by spirit of wine and spirit of turpentine united. They were washed with spirit of sal ammoniac, and solutions of pot-ash, for a longer time than would destroy common oil colours, without being injured.

They dry very
well.

They dry as well in damp as in dry weather, and without any skin upon the surface. They are not liable to crack, and are of a flinty hardness; whence it appears that this vehicle possesses every desirable property, and it is presumed may be a discovery of some importance to artists.

Gum copal nearly
as good as am-
ber, and rather
brighter.

Having succeeded thus far with amber, I tried the same experiments upon solutions of gum copal, which is nearly as hard and insoluble as amber itself. The result of these was the same as the former, except that with the copal the colours were something brighter than with amber. As it is extremely troublesome to dissolve the copal and amber, I tried those solutions of them in oil which are sold in the shops. When good, I found them to answer as well as my own. This is a great convenience, as many might be deterred by the difficulty of preparing this vehicle, who may willingly use it, as it is thus to be procured without that trouble.

The Method of using the Solution of Amber or Copal, as a Vehicle for painting.

Instructions for
the use of am-
ber or copal as a
vehicle for paint-
ing.

The cloth or other substance to be painted on, should be prepared with some colour fully saturated with drying oil; or it will be better done with the same vehicle to be used in painting.

painting. If it is not fully saturated, it will absorb some of the vehicle from the colours, which is commonly termed the colours sinking in.

All the colours which require grinding, should be previously ground in spirit of turpentine. All the pure parts should be tempered with such a quantity of the vehicle as will enable them to lie on the pallet. The white should be tempered as stiff as possible. All the tints should be made by mixing the colours so prepared without any more of the vehicle, but they should be diluted with spirit of turpentine, if necessary for working. Grinding colours, &c.

If the ground is properly prepared, and the above caution observed in tempering the colours, it will be found that all the dark colours in the picture will bear their full tone, and have a demi-transparency, which increases their native brilliancy, without the dingy appearance so common in ordinary oil-painting. The admixture of white increases the body of the colours progressively, till there will be left in the lightest parts, only so much of the vehicle as will bind the colours, and give them their full tone, but with very little of a shining appearance. When the picture is perfectly dry, it should be varnished with a mastic or similar varnish. Perhaps the best would be copal varnish made by solution in spirit of turpentine, or spirit of wine. Consequences or effects.

The rationale of this vehicle seems to be this: the amber and copal, when dissolved in oil, form a homogeneous mass, which dries by inspissating, instead of skinning over, like the common drying oils, which consist of heterogeneous parts, some of which separate and dry on the top. Observations.

As the amber and copal are not soluble in any of the menstrua which dissolve most resinous substances, pictures painted with them cannot be injured, if cleansed with those menstrua: and as they are extremely hard, and the most durable substances of their class, they protect the colours from every kind of injury, more effectually than any other known vehicle.

Conjectures to show that the foregoing vehicles were used by the older painters.

Lomazzo.

No. III. *Conjectures tending to show that the Vehicle which I have described, is similar in Principle, if not identically the same, as that used by several of the older Painters, who were eminent for their Skill in colouring.*

Lomazzo, an eminent painter, and pupil of Leonardo da Vinci, published a Treatise on Painting, in which it is mentioned that linseed or nut oil was generally used for painting: he likewise observes, that powdered glass was used as a dryer. As Lomazzo was blind when he published his treatise, he could have no motive for keeping any thing which he knew secret; whence it is to be concluded, that those oils were generally used for painting in his time, and that he knew of no exceptions to the practice.

Leonardo da Vinci.

In one part of L. da Vinci's Treatise on Painting, he mentions *nut-oil and amber*. As we know that amber gives peculiar brilliancy to colours, that L. da Vinci was peculiarly celebrated for the richness of his colouring, and are informed from his own writings that he was acquainted with solution of amber in nut-oil, it is to be presumed that was the vehicle he used. If this supposition is not to be admitted, we must believe that he knew how to dissolve amber in nut-oil (a process at that time both tedious and troublesome), without knowing the best use to which he could possibly apply it.

Leonardo's biographer says, "When he was at Rome, Leo X. resolved to employ him. Leonardo hereupon sets himself to the distilling of oils, and the preparing of varnishes to cover his paintings withal: of which the Pope being informed, said, pertly enough, that he could expect nothing of a man who thought of finishing his works before he had begun them. Leonardo therefore left Rome without having been employed."

I must beg leave to dissent from his Holiness's opinion. If my idea of Leonardo's vehicle be just, it was natural for him to begin the preparation of it as soon as he knew that he was to be employed as a painter: and as the spirit of that time led every one who made any useful discovery, to preserve it as a valuable secret, it was equally natural for him to account for his employment by saying that he was preparing varnishes. Whatever his secrets were, they remained unknown to the world till 1651, when his Treatise on Painting was published.

The

The next intimation of solutions of amber which I have obtained is from the works of Boyle, who gained much of his information from Italian chemists; whence it is evident that the knowledge of this preparation is of long standing in that country; and its use, if it was used at all in the arts, is to be sought for in the works of Italian artists.

Whoever examines the Venetian pictures with attention, considers that the best artists of that school were remarkable for the facility with which they worked, and reflects on some passages in Lomazzo, will be disposed to admit that the peculiar skill of the Venetian painters depended on three circumstances, viz. the colours they used, their method of using them, and the vehicle they worked with. Of the first, Lomazzo gives positive information: the second can never be known without information equally positive; but of the vehicle some knowledge may be obtained by way of analysis. Till that knowledge is obtained, I may perhaps be excused for hazarding the following conjectures.

If my experiments have not misled me, I am entitled to draw the following conclusions from them. Wherever a picture is found possessing evidently superior brilliancy of colour independent of what is produced by the painter's skill in colouring, that brilliancy is derived from the admixture of some resinous substance in the vehicle. If it does not yield on the application of spirit of turpentine and spirit of wine separately or together, or to such alkalies as are known to dissolve oils in the same time, it is to be presumed that vehicle contains amber or copal, because they are the only substances known to resist those menstrua.

I have been told, and some experiments of my own prove the information to be true, that the Venetian pictures, considered with respect to vehicle, are of two kinds: for some are extremely hard, and not at all affected by any of the above menstrua; others are similar in colour, but so tender that it is scarcely possible to clean them without injury, and in that respect are little superior to mere turpentine colours. The first, in consequence of the data which I have laid down, incur the suspicion of being painted with amber or copal, but how are we to distinguish with which?

As each of these substances resists equally the common menstrua, perhaps the distinction can only be made by a ^{Attempt to} ~~ascertain~~ ^{make the distinction between} ~~ascertaining~~ ^{taining} them.

Robert Boyle.

On the pictures of the Venetian school.

Superior brilliancy of colour from the vehicle.

which appears to have been amber or copal.

...taining the date of the picture. For example:—if it is found to have been painted before copal was known in commerce, it may safely be said to have amber for its basis; but if it has been painted after that period, I know of no method of distinguishing which of the two was made use of. As copal could not have been known, as an article of trade, before the seventeenth century, it follows that all pictures painted before that period, and possessing the properties I have described, must have amber for the basis of their vehicle. As this exception necessarily includes all the Venetian artists of the first class, we are therefore authorised to conclude that, if the works of these artists can bear the test of the menstrua I have mentioned, amber was the basis of the vehicle with which they were painted.

Ancient recipe, in which turpentine was the solvent of copal.

I once saw a recipe for dissolving copal, said to have been brought from Venice towards the close of the last century. The process was, to melt Venice turpentine upon the fire, to add gradually copal powdered, stirring them together to be united in fusion, and afterwards spirit of turpentine, in order to dilute it to the consistence of varnish. I tried this process, but it did not succeed.

It was not common, but Chio turpentine.

Upon inquiry I found that the Venice turpentine of the ships was only common resin, dissolved in spirit of turpentine to a proper consistence; whence the cause of my failure was evident. Reflecting on the commercial pursuits of the Venetians in the fifteenth and sixteenth centuries, I was led to conjecture that the substance called originally Venice turpentine was the product of some country intimately connected with them. Pursuing this idea, I procured, with much difficulty, some Chio turpentine, repeated my experiment, and succeeded completely. Besides the property of uniting easily with copal, it had others that excited my attention. Common resins, if exposed to fire, burn with extreme fierceness and rapidity; but when some of this was laid on the point of a knife, and held in the flame of a candle, it melted and dropped down before it began to burn. It emitted a peculiarly grateful smell; was of a most beautiful pale gold colour; was more brilliant than any turpentine I had ever seen; and when diluted to the consistence of varnish, perfectly resembled in colour, a solution of copal which I made in spirit of turpentine with camphor.

This turpentine differs from the common sort.

I showed

I showed some of this to a gentleman who was conversant in such subjects. He told me that, when at Venice, he frequently rubbed pictures violently with his handkerchief, to try if he could discover what they were painted with; and when so rubbed, they smelt exactly like what I then produced to him.

Additional probability that the Venetians used it.

As I had previously perfected what I thought to be a superior vehicle, with which this could not vie in hardness and durability, I did not prosecute my experiments with this any farther; but as it unites rapidly with copal, and possesses all its visible properties, I may be permitted to conjecture that it would have similar effects when mixed with colours: and if there was any second, inferior, and common vehicle, similar in its visible properties to the last, and so much within the reach of the most ordinary painters, as to give their works one common mark with those of the first artists, it would be difficult to point out a substance more likely to afford it than this which must have been common in their own country, since its name is still attached to substances of the same class throughout Europe, though its real properties are now but little known.

This last compound less hard and durable than the former.

If this was the basis of the common Venetian vehicle, it might have been used with or without oil. If the latter, the works of the common Venetian painters must have been mere varnish painting: if the former, it must have been compounded with the oil, according to the principles I have already explained. I am inclined towards the latter opinion, from having heard an observation attributed to Bombelli, a celebrated Venetian painter, who said, "*That he wished his pictures to dry as fast as possible, that the oil in them might not rise to the surface, and turn yellow.*"

It might be used with oil or without.

To this conjecture it may be objected that turpentine and compounds from them do not dry well. I am not prepared to answer this objection, as I have made no experiments relative to it; but it certainly is not conclusive, as such compounds may not dry well in this country, though they may in the warm climate of Italy.

In the *Maniere d'imprimer les Tableaux*, published by Le Blond at Paris, 1740, is a recipe for the varnish he used on the coloured prints executed by him, in this country, before he

Varnish of Le Blond.

he went to France. It is as follows:—"Take four parts of balsam of capivi and one of copal. Powder and sift the copal; and throw it by degrees into the balsam of capivi, stirring it well each time it is put in: I say each time; for the powdered copal must be put in by degrees, day after day, in at least, fifteen different parts. The vessel must be close stopped, and exposed to the heat of the sun, or a similar degree of heat, during the whole time; and when the whole is reduced uniformly to the consistence of honey, add a quantity of warm turpentine; *Chio turpentine is the best.*"

its durability
and excellence.

Le Blond's prints were long neglected, and are now forgotten. Whatever difference of opinion may prevail respecting them, there can be none respecting his varnish, as I have seen some of these prints in perfect condition, notwithstanding they had been thrown carelessly about for nearly sixty years.

His intelligence
probably from
Italy.

Le Blond was a pupil of Carlo Maratti. He died at a very advanced age, leaving behind him the character of an ingenious projector. It is probable that he might collect much information analogous to his pursuits during a long life; but it is more probable that he obtained much of it where he received his education. Thus, wherever we find notices of the use of these substances in the arts, they invariably lead us towards Italy, where they certainly were first known.

I have thus detailed the circumstances which impress me with a conviction that the vehicle I have offered to public notice is, in substance, the same as that used by the best colourists of the Italian schools. What impression the facts I have enumerated may make upon others I know not: but still the truth of my opinion must be determined by experience; for it would be of small consequence to prove that this vehicle was used in former times, unless it can likewise be made evident that it will be useful to the present race of artists.

Yours, &c.

TIMOTHY SHELDRAKE,

Strand, February, 1801.

VI.

On the Native and Artificial Sulphurets of Iron, by PROFESSOR PROUST.*

IN a former memoir † I remarked, that the reason why the acids which easily dissolve the artificial sulphuret, do not exercise the same power on the native sulphuret, consists in an excess of sulphur in the latter, which art has not yet succeeded in combining with iron. Hitherto in fact, I have not presumed that our imperfect means could approach sufficiently near those of nature for us to hope to imitate the formation of pyrites. Accident, however, has lately removed this difficulty.

Why native sulphurets of iron are not soluble in acids like the artificial.

I heated, without any particular attention to the quantities, a mixture of about ten ounces of sulphur and of filings, in order to supply my laboratory; and judging by the colour that the quantity of sulphur was insufficient, I thought proper to add a new dose. In consequence of this, the crucible was made nearly red hot; but not to fusion, because it is more convenient for use to obtain it in the state of powder. When I afterwards tried to dissolve it in an acid properly diluted, I was somewhat surprized to find that it did not afford sulphurated hydrogen. It was to no purpose that I altered the strength of my acid, for I obtained no gas. This unexpected result shewed the possibility of forming pyrites by art.

Iron pyrites formed by art.

Since pyrites becomes soluble as we have seen, only by depriving it of the sulphur which exceeds its point of saturation, it appeared to me, that I should first endeavour to restore all its qualities by producing its first state from a similar excess of sulphur; and this in fact succeeded.

Roasted pyrites restored by giving it the requisite dose of sulphur.

I mixed an indeterminate quantity of sulphur with four hundred grains of the pyrites of Soria, deprived of its excess by distillation, and I heated the mixture in a retort beside another, which also contained four hundred grains of pyrites in the crude state. My object in this last arrangement was to obtain a kind of thermometer, to prevent my giving too great a degree of heat to the first retort; that is to say, not to ex-

Experiments.

* Journal de Physique, Pluviose X year.

† Philos. Journal, I. 109.

pose this second dose of sulphur, which a new attraction was to add, to that temperature which would constitute the first point of saturation with regard to iron.

At a given heat the super-abundance of sulphur was carried off by distillation, after which the two retorts being kept an hour longer at the same temperature, did not exhibit the slightest vapour of sulphur.

Regenerated pyrites.

The regenerated pyrites was pulverulent; a proof that it had retained no portion of sulphur beyond the point of saturation, otherwise I should have found it in a pasty state, having the figure of the retort.

Characters.

It had resumed the greenish yellow colour, which is that of crude pyrites when pounded, whereas its tinge was before dull and blackish like sulphurated iron, which is capable of affording hydrogen. Its weight was found to be five hundred and four grains, that is to say, the distilled pyrites had resumed twenty-six pounds of sulphur per quintal.

Proportions of parts.

Observations on the quantities; to prove that the artificial pyrites is the same as the native.

By inspecting the former memoir, we see that the mean product of two distillations, each consisting of four hundred grains of pyrites, was three hundred and eighteen for the residue, and seventy-eight for the sulphur, to which must be added three or four for what was carried away by the gas, so that in the quintal we have seventy-nine and a half residue, and twenty and a half sulphur. According to this report, four hundred grains of the residue, or distilled pyrites, ought to have taken only ninety-eight and a fraction of sulphur, whereas the result of our experiment gives one hundred and four. The difference being no more than one and half per quintal, may be ascribed to the inaccuracy of experiment, as well as the nature of the pyrites, which is not an homogeneous combination; for besides the clay and the sand, it often contains a small portion of oxide of iron, which may have caused the fixation of a somewhat greater quantity of sulphur than the loss it suffered by distillation.

Trial of the regenerated pyrites by acids; which did not dissolve it.

I afterwards examined my regenerated pyrites with a sulphuric acid of ten degrees by the areometer of Baumé, which acid dissolves the distilled pyrites very well; but I obtained only a few ounces of sulphurated hydrogen. I afterwards heated the mixture; it afforded a slight portion of gas, after which the pyrites remained without alteration. Long continued boiling was ineffectual for saturating my acid.

With

With the muriatic acid I succeeded in forming a small portion of gas; but its action soon ceased, and it was equally impossible for me to saturate the acid. It was of twelve degrees of the areometer, and the powder preserved its colour, The natural pyrites treated in the same manner does not afford the slightest suspicion of gas; but we must not overlook that art cannot give to its compounds that aggregate condensation which is one of the greatest obstacles to solution. Morveau and Fourcroy have given us very striking proofs in the resistance they found in dissolving the native oxides of iron and tin.

The aggregation of native pyrites in some measure defends it.

The pyrites not being, as I have observed, an homogeneous combination, it is evident that we could not expect to discover the true proportions of sulphur wherein iron could attract by these previous experiments; for which reason I made the following.

These experiments could not shew the proportions of sulphur.

In order to depend in the first place upon pure filings, we must begin by heating them gently, and for a considerable time in a glass retort. Under these circumstances, it is not a little remarkable to observe, that filings cleaned with the magnet, and kept in well-closed bottles, afford nevertheless a very ammoniacal water, and even muriate, if I am not deceived in the taste of the fluid which escapes.

Other experiments.

Iron filings afford ammonia.

One hundred parts of filings heated to a low red heat in a retort, and sulphur being let fall upon them, acquire an incandescence which was noticed even by the ancient chemists; but they do not become saturated. The increase is found to be only twenty or thirty parts. After pounding the product, then mixing it with sulphur, and exposing it to a red heat, a result is obtained, which weighs pretty constantly 159; but which, I think, may be set at 160, on account of the impurities of the iron.

Direct combination of iron and sulphur.

First degree of sulphurization. 60 parts of sulphur to 100 iron.

The product is iron sulphurated to the first degree. It may be fused, and even melts in the retort, if harpsichord wire be used, and maintains itself without alteration. Its colour is metallic, but dull and considerably different from the golden colour of the pyrites. In a word, it is the sulphuret which is proper to afford sulphurated hydrogen gas.

Its appearance.

In order to discover the excess which this sulphuret can still absorb, provided its temperature be less elevated, I treated two hundred grains of filings with the before-men-

Second degree, 29 sulphur to 100 iron.

tioned precautions, and obtained three hundred and eighteen grains of sulphuret. When taken out of the retort it was mixed with a new dose of sulphur, and heated beside another which contained crude pyrites. The super-abundant sulphur passed in distillation, and the retorts were kept for an hour at the same heat. The result of this operation was an artificial pyrites, weighing three hundred and seventy-eight grains. It was pulverulent, which proved that it had retained no more sulphur than its attraction could take up.

Its appearance
and habitudes.

Its colour was no longer blackish, but greenish yellow. When treated with the acids, its habitudes were precisely the same as those of the regenerated pyrites; and lastly, it differed from the native pyrites only by the want of that density which the latter received from their humid crystallization.

Consequences.

Recapitulation
of the facts,
with observa-
tions.

From the foregoing facts it follows, that iron can fix sixty per cent. of sulphur by a considerably elevated temperature. This proportion constitutes iron sulphurated to the minimum.

Sulphuret at
the minimum
10 sulph. + 6
iron.

By a lower heat it can also attract a quantity which is equal to the half of this weight; and this result is iron sulphurated to the maximum, or with ninety parts of sulphur. If this last combination be exposed to the temperature which formed the first, it returns to that state; that is to say, it returns to the minimum of sulphuration, by giving out all the sulphur it was capable of fixing above the proportion of sixty parts for each quintal of iron. Iron sulphurated to the maximum is the pyrites, and possesses all its properties excepting density.

Sulphuret at the
maximum 10
sulph. + 9 iron.

Pyrites reduced
to the minimum
by heat with iron
filings,

In order to render the highly-sulphurated iron fit for affording hydrogen, it need only be heated with half its weight of filings.

or by distillation,

To apply pyrites to the same use, the same treatment may be adopted, or they may be distilled, in order to deprive them of that portion of sulphur which constitutes the difference between the sulphuret at the minimum, and that at the maximum.

not found native.

The mineral kingdom has not yet presented iron sulphurated to the minimum. In the yellow coppery pyrites, which is a compound of two sulphurets, the iron is always at the maximum of its saturation, and accordingly these ores resist all acids, except such as can oxide the excess of sulphur.

Copper in both
states ?

I shewed in my former memoir, that the native sulphuret of copper is usually found with an excess of sulphur amounting

ing to 14 or 15 per cent; it is very possible, that this metal may in its saturation follow the same law as iron, which requires to be examined; but only in the sulphurets which are pure; for in those which are complicated, or those in which the sulphuret of copper is an integrant part, I have found it without excess of sulphur.

The yellow copper ores, or the native union of the two sulphurets of copper and iron sulphurated to the maximum, afford by distillation less sulphur than the simple pyrites, because the sulphuret of copper in this mixture has no excess. The beautiful copper pyrites of Avalar in Biscay, affords no more than one twelfth of sulphur by distillation.

Copper pyrites, are mixed: their habitudes.

If these pyrites be fused with a portion of potash, the excess of sulphur unites to this alkali, and the sulphuret of iron is reduced to the minimum. The aqueous sulphuric acid may then be used to analyse them. It dissolves all the sulphuret of iron without affecting that of copper, which in that case presents itself with a deep blue colour, which is one of its characters. By this means we may obtain the proportion of the two sulphurets which compose this kind of mineral. But when we know the quantity of copper contained in a copper pyrites, we always know that of the sulphuret of the metal, because in nature, as well as in art, copper never takes up more or less than 28 grains of sulphur per quintal.

Analysis of the compound pyrites.

If we wish to ascertain the quantity of sulphuret in a copper pyrites, its nitric solution must be precipitated by sulphurated hydrogen, and the precipitate ignited in a retort. The product always represents the real quantity of sulphuret originally combined in the mineral.

Copper in nature and also by art takes up exactly 28 per cent. of sulphur.

Native Sulphuret of Manganese.

This sulphuret has not been noticed by mineralogists as far as I know. I discovered it some time ago in certain specimens of gold-ore from Nagayag.

Native sulphuret of manganese.

The gangue of the piece which afforded this sulphuret is a carbonate of manganese mixed with quartz, like that of the sulphuret of Tellurium. It does not present metallic crystals of this metal, but a multitude of points, which under the magnifier appear to be an assemblage of pyritous parts. This mineral treated with the aqueous sulphuric acid, affords a

History and description.

Treatment.

very abundant mixture of carbonic gas and sulphureous hydrogen. The first arises from the decomposition of the carbonate, and the second from that of the sulphuret.

This new sulphuret of manganese very easily known.

It is extremely easy to ascertain the existence of this new sulphuret. First, because among all those we are acquainted with there is none which is so easily decomposed, and with such abundance of this gas. Secondly, because the artificial sulphuret of manganese obeys the action of this acid with the same celerity. And thirdly, because we do not discover any thing else in the solution but oxide of manganese, and a very minute quantity of that of iron. The specimen does not contain either gold, tellurium, lead, or any other metal.

Ore of tellurium.

In the true ore of tellurium, I found that the sulphurets of lead and of tellurium are combined together, and that the gold is native, and not at all mineralized.

Is the manganese, metallic, or oxidized?

I shall add nothing further respecting the species of sulphuret here mentioned, because I have not enough to decide whether the manganese exists in the state of oxide or metal. If it be an oxide, its sulphuret may probably derive from a very condensed aggregation the power of eluding the activity of the atmospheric oxygen; for the artificial sulphuret passes with considerable speed to the state of black oxide mixed with sulphate.

Concerning the Di-oxidation of Iron.

On di-oxidation. When we consider the facility, or low temperature which several oxides require to return to the metallic state, we cannot help thinking, that those which resist the effort of our furnaces, would also become di-oxidized, if it were possible for us to heat them to the necessary degree.

Incident respecting the reduction of iron by mere heat.

In order that iron may no longer remain in the class of oxides which are incapable of reduction without the assistance of charcoal, I think it proper to record in this place an experiment, for which I am indebted to the friendship of Naft, manufacturer of porcelain, at that time established in the Fauxbourg St. Antoine.

A bar of iron oxidized nearly all through was reduced by heat in a porcelain furnace.

The mouth of his furnace for second firing, which is above the furnace itself, was supported by a bar of iron about an inch in diameter. The oxidation had approached so near its centre, that being no longer supported but by a thread of iron

of

of one line in diameter, it fell and broke to pieces. I collected the fragments, and separated the metallic thread by a slight blow. The oxide differed in no respect from the scales, which are separated from iron when forged.

I placed eight ounces of this oxide in a porcelain crucible inclosed in one of the faggers below. I was not ignorant that nothing more is necessary, than the fall of a single nail to destroy a whole pile. The following was the result: we found the crucible and the fagger perforated; the iron reduced and well fused had bedded itself in the floor of the furnace. We could not separate it but by blows of the chissel. It was not brittle. Did this iron melt merely by the intensity of the fire, or did it oxigenate some charcoal, or decompose the calcareous part of the pottery, &c. It would I think be an interesting object to ascertain this. I kept this iron for a long time, and do not at present know what is become of it. I was not sufficiently aware at the time how much the theory of chemistry might be interested in its examination. It is much to be desired that the experiment should be repeated.

Observations and queries.

Concerning Wax.

I think I may announce to you, that wax exists in the green fecula; I had before found it in that of opium, and expect to find it ready formed in the fecundating powder. I shall at some future opportunity treat it in the same manner as the fecula.

Wax in the green fecula of vegetables.

VII.

General Observations on the Causes which influence the Weather in England, and the popular Methods of judging of the Weather.
By JAMES CAPPER, Esq. formerly Colonel and Comptroller General of the Army and Fortification Accounts on the Coast of Coromandel*.

THOSE who are furnished with proper instruments, and who carefully observe the information they afford, will not often be mistaken in their judgment of the changes of the weather;

Philosophical instruments are of great utility for judging of the weather;

* From his "Observations on the Winds and Monsoons" 4to, London, 1801.

weather. The barometer, the thermometer, the hygrometer, and the electrometer, will generally give us timely notice of any material changes in the state of the atmosphere. But before we consider the best, or at least the usual modes of employing these instruments, we will beg leave to mention some

yet the common remarks of peasants are valuable.

common remarks of the peasantry, whose professions requiring them to live much in the open air, their opinions merit very great attention, being the result of local observation, continued from father to son, and verified from the experience of many ages. Amongst the first of these is one, now established into a proverb, that a rainbow in the morning is the shepherd's warning, but a rainbow at night is the shepherd's delight.

Morning rainbow denotes rain; evening rainbow, fair weather.

Explanation of the prognostic.

With a west wind the morning rainbow to the west denotes coming showers; evening rainbow showers past.

In a country with the sea or ocean to the westward, and the wind from the same quarter, this opinion is likely to be true. If therefore, the clouds to the westward in the morning are saturated with moisture, which they must be to produce a rainbow, as these clouds proceed from the W. towards the E. they probably will produce rain; whereas, on the contrary, when the sun sets perfectly clear, and the clouds to the eastward are moist, it is a proof that the wet clouds are past, with a westerly wind, and the shepherd therefore may reasonably expect fine weather on the following day.

Rain, if from the east, lasts 24 hours.

Explanation.

When it rains with an east wind, it probably will rain for twenty-four hours. This is another observation, which seems to me applicable to countries situated as above-mentioned, with land to the eastward; for, in general, the weather is dry in these countries with an east wind, but when the cohesion of the air and water is broken, the rain will not be violent, but of long duration.

Weather usually clears at noon, but if not, then at sun-set.

Explanation.

The weather generally clears at noon, but when it rains at mid-day, it seldom clears up again till sun-set. The air, when dry and warm, continues to absorb and retain the moisture continually evaporated from the earth; as therefore the sun advances towards the meridian, and for an hour or two afterwards, he dries and warms the air; and consequently the rain is likely to cease at that time. But if there should be so much water in solution in the atmosphere, that the heat of the sun is not sufficient to produce these effects, in that case the rain will probably continue some hours longer.

Violent

Violent winds generally abate towards sun-set.

Strong winds
abate at sun-set
because the
change of tem-
perature changes
the cause.

If we admit that wind is only a current of air put in motion by the rarefaction of the atmosphere in some particular place, and that this current of air is moving towards the point of rarefaction to restore the equilibrium, we must suppose, that as the sun declines the rarefaction will diminish, and consequently the velocity of the wind decrease. But this observation, in my opinion, rather applies to the temperate than to the torrid zone; for in whirlwinds and hurricanes the contrary may very often occur.

When the wind follows the course of the sun, it is generally attended with fair weather. This frequent and regular change of wind, which is never more than a moderate breeze, proves that there is no point of considerable rarefaction near, and therefore, the current of air follows immediately the sun's course: it always happens in summer, but very seldom when the sun's meridian altitude is less than 40 degrees.

Weather steady
when the wind
follows the sun.

The changes which take place in the atmosphere are principally marked by the rising and falling of the barometer, which apparently is caused by heat and cold, the hands with which nature performs her meteorological operations: by the former the atmosphere is rarefied, and consequently becomes light; by the latter it is condensed, and consequently becomes heavy. Hence probably the old remark, that a storm generally follows a calm; for during a calm the air is rarefied and expanded, and the cold air will rush forward in a strong current to restore the equilibrium, and necessarily produce what is generally called a gale of wind, the violence of which also will of course be in proportion to the degree of its preceding rarefaction.

A storm follows
a calm;

because the air
over any spot is
more heated
when quiescent,
and the sur-
rounding air
rushes in.

For these reasons, the barometer falls suddenly whilst the air is expanded before a gale of wind, and rises again gradually as the condensed air returns, and the gale in like manner by degrees subsides.

The barometer
confirms the
fact.

It must however be observed, that an extraordinary fall of the mercury will sometimes take place in summer, previous to heavy showers of rain, particularly if attended with thunder and lightning; but in spring, autumn, and winter, the sudden extraordinary descent of the barometer indicates principally violent wind.

Summer showers
denoted by a fall
of the barometer.

Upon

Why the barometer varies little near the equator, and very much in high latitudes.

St. Helena.

Madras.

S. of Europe.

England.

Petersburgh.

Upon these principles likewise we may account for the rise and fall of the barometer in the different zones. In the torrid zone, particularly at St. Helena, and the islands of the Pacific ocean, it seldom varies more than three tenths; at Madras about five-tenths; in the south of Europe not more than one inch and two-tenths; in England it varies two inches and a half, and in Petersburgh three inches four-tenths. In the two first, the temperature of the atmosphere is not subject to much variation, and never to any great degree of condensation. In the third, reckoning from the tropics to the latitude of 40°, the atmosphere may sometimes be suddenly condensed by currents of cold air from the N. and still more so in England. But the greatest variation must necessarily take place on the continent to the northward, where, during the summer, the weather is as hot as within the tropics; and, in winter, the thermometer for many weeks, continues several degrees below the freezing point.

Thermometer;
hygrometer;
electrometer.

The thermometer also, which measures the degree of heat in the air near the earth, will contribute towards denoting when changes are likely to take place in the lower regions of the atmosphere; the hygrometer distinguishes the quantity of moisture in the atmosphere, and the electrometer will point out the quantity of electricity which prevails in it.

The barometer little esteemed by farmers, because the words engraved thereon are fallacious.

The words generally engraven on the plates of the barometer rather serve to mislead than to inform; for the changes of the weather depend rather on the rising and falling of the mercury, than on its standing at any particular height. When the mercury is as high as fair, or at 30 degrees, and the surface of it is concave, beginning to descend, it very often rains; and on the contrary, when even the mercury is at 29 degrees, opposite to rain, when the surface of it is convex, beginning to rise, fair weather may be expected. These circumstances not being known, or not duly attended to, is the principal cause that farmers and others have not a proper confidence in this instrument.

Daily change in the barometer, and why.

It must also be observed, that *ceteris paribus*, the mercury is higher in cold than in warm weather, and commonly early in the morning, or late in the evening, than at noon, which seems occasioned by the obvious causes of the atmosphere being condensed by the cold of the night, and rarefied by the heat of the day.

The

The following observations of Mr. Patrick seem confirmed by experience. The old observations of Patrick are good.

1. The rising of the mercury presages, in general, fair weather, and its falling foul weather, as rain, snow, high winds, and storms.

2. In very hot weather the fall of the mercury indicates thunder.

3. In winter the rising presages frost; and in frosty weather, if the mercury falls three or four divisions, there will certainly follow a thaw; but in a continued frost, if the mercury rises, it will certainly snow.

4. When foul weather happens soon after the falling of the mercury, expect but little of it, and, on the contrary, expect but little fair weather, when it proves fair shortly after the mercury has risen.

5. In foul weather, when the mercury rises much and high, and so continues for two or three days before the foul weather is quite over, then expect a continuance of fair weather to follow.

6. In fair weather when the mercury falls much and low, and thus continues for two or three days before the rain comes, then expect a great deal of wet, and probably high winds.

7. The unsettled motion of the mercury denotes uncertain and changeable weather.

But to these remarks may be added, that, when the barometer suddenly falls two or three tenths, without any material alteration in the thermometer, and the hygrometer is not much turned towards moist, a violent gale of wind may be expected. When the hygrometer inclines far towards moist, with only a trifling descent in the barometer, it denotes a passing shower and little wind; and when the barometer falls considerably, and the hygrometer turns much towards moist, the thermometer remaining stationary, and rather inclined to rise than fall, both violent wind and rain are likely to follow in the course of a few hours.

Prognostics of violent wind; of showers; or of a strong gale of wind and heavy rain.

General or common Prognostics of the Weather.

Amongst these we may reckon such as are derived from birds, beasts, insects, reptiles and plants, to which might be added great part of the wood work in houses, as doors, windows, window shutters, &c.

Common prognostics.

Birds

- Birds dress their feathers against rain,** Birds in general retain in the quill part of their feathers a quantity of oil, which when they feel an extraordinary degree of moisture in the atmosphere, they express, by means of their bills, and distribute it over their feathers, to secure their bodies against the effects of an approaching shower.
- and swallows fly low.** Swallows in pursuit of the flies and insects, on which they prey, keep near the earth in wet weather; and in dry weather, from the same cause, they fly much higher.
- Cattle feed hastily, and go to shelter.** Domestic animals, as cows and sheep, but particularly the latter, on the approach of rain, feed with great avidity in the open field, and retire near the trees and hedges as soon as they are satisfied. In fine weather they graze and lounge about, eating and resting alternately with apparent indifference.
- The pimpernel and dandelion much affected by the approach of wet weather.** The pimpernel, commonly called peep-a-day, or shepherd's weather-glass, closes its leaves before rain; and the down of the dandelion is much affected by moisture.
- Wood work swells.** All wood, even the hardest and most solid, swells in moist weather. The vapours insinuate themselves into the pores of trees, and also into the wood-work of houses.
- Insects alter their conduct in various ways.** Insects and reptiles of all kinds seek or avoid rain according to their respective habits, by these means giving notice of every change of weather.
- The drains emit a smell;** It is a well known fact, that before rain, particularly in summer, a strong smell is perceived from drains and common sewers, as well as from every other body emitting a great quantity of effluvia. During fair weather, even in the summer, the atmosphere readily absorbs all the vapours and exhalations from the earth until it is completely saturated, and consequently the effluvia from the bodies which emit them, will then be confined and ascend in a narrow compass, like the smoke of a chimney in dry weather, almost perpendicularly; but when the air is saturated with moisture, and becomes rarefied and expanded, as it always does before rain, the volume of air containing the effluvia will be extended horizontally, and diverge from these different bodies as from a centre, and will be sensibly perceived on all sides, but will of course be most perceptible on that to which the current of air or wind moves.
- Vapors seen in winter over water.** In winter, when the thermometer is between 34 and 40 degrees, the air being in a state of condensation, and the running

On this subject see Philos. Journal, quarto, IV. 135.

running water being warmer than the land, a mist or fog may be seen rising above the rivers, particularly when the air is cold and clear; but this vapour is no longer visible when the river is frozen, for though the ice be subject to evaporation, it does not yield so much vapour as water, and the water, in parting with its caloric in the moment of freezing, warms the surrounding air.

To the philosopher all objects in nature, both animate and inanimate, may afford both amusement and instruction, particularly in meteorology; but to observe them with due attention, we must quit the busy scenes of life; "and thus our
"lives exempt from public haunts, find tongues in trees,
"books in running brooks, sermons in stones, and good in
"every thing."

Amusement and instruction from the solitary contemplation of nature.

VIII.

Description of a Cheap, Simple, and Portable Instrument, for determining the Positions of Objects in taking a Picture from the Life. By R. L. EDGEWORTH, Esq.

THAT active and intelligent philosopher and journalist, Cit. Piçet, author of the *Bibliothèque Britannique*, on his late return from London to Paris, presented various instruments brought from this country to the National Institute of France. Among them was the instrument here to be described, and since published in the *Bulletin des Sciences*; on inspection of which my attention was excited to my notes, where, among other communications to be made to my readers, I find this instrument, as published in the excellent work "On Practical Education" by Maria Edgeworth and her father, to whom I have ascribed the contrivance in the title. The authors of the *Bulletin* affirm, that it was invented and executed by the children under the parental care of Miss Edgeworth; but I find no such intimation in the original work. The instrument of Professor Piçet is in various respects inferior to that which I have here copied; inasmuch that Cit. Cloquet has proposed an amendment, for giving the index an horizontal position when it is required to transfer the observation to paper, which is in fact less effectual than the provisions made for that purpose,

Instrument for perspective presented to the National Institute;

was not as perfect as the original invention.

pose and for fixing the true instrument. After this preface, I shall proceed to copy without farther remark, page 460.

Advantages of early habits of delineation, and the inspection of drawings, &c. of machines.

“ An early use of a rule and pencil, and easy access to prints of machines, of architecture, and of implements of trades, are of obvious use in this part of education (mechanics). The machines published by the Society of Arts in London, the prints in Desaguliers, Emerson, le Spectacle de la Nature, Machines approuvées par l'Académie, Chambers's Dictionary, Berthoud sur l'Horlogerie, Dictionnaire des Arts et des Mèti-
 ... tiers, may, in succession, be put into the hands of children. The most simple should be first selected, and the pupils should be accustomed to attend minutely to one print before another is given to them. A proper person should carefully point out and explain to them the first prints that they examine; they may afterwards be left to themselves.

Method of conveying the requisite previous information,

“ To understand prints of machines, a previous knowledge of what is meant by an elevation, a profile, a section, a perspective view, and a (*rue d'oiseau*) bird's eye view, is necessary. To obtain distinct ideas of sections, a few models of common furniture, as chests of drawers, bellows, grates, &c. may be provided, and cut asunder in different directions. Children easily comprehend this part of drawing, and its uses, which may be pointed out in books of architecture; its application to the common business of life is so various and immediate, as to fix it for ever in the memory; besides, the habit of abstraction, which is acquired by drawing the sections of complicated architecture or machinery, is highly advantageous to the mind. The parts which we wish to express are concealed, and are suggested partly by the elevation or profile of the figure, and partly by the connection between the end proposed in the construction of the building, machine, &c. and the means which are adapted to effect it.

The art of perspective requires a contemplation of the mere picture of external objects.

“ A knowledge of perspective is to be acquired by an operation of the mind, directly opposite to what is necessary in delineating the sections of bodies; the mind must here be intent only upon the objects that are delineated upon the retina, exactly what we see; it must forget or suspend the knowledge it has acquired from experience, and must see with the eye of childhood no farther than the surface. Every person who is accustomed to draw in perspective, sees external nature, when he pleases, merely as a picture: this habit contributes much

to form a taste for the fine arts; it may, however, be carried to excess. There are improvers who prefer the most dreary ruin to an elegant and convenient mansion, and who prefer a blasted stump to the glorious foliage of the oak.

"Perspective is not, however, recommended merely as a means of improving the taste, but as it is useful in facilitating the knowledge of mechanics. When once children are familiarly acquainted with perspective, and with the representations of machines by elevations, sections, &c. prints will supply them with an extensive variety of information; and when they see real machines, their structure and use will be easily comprehended. The noise, the seeming confusion, and the size of several machines, make it difficult to comprehend, and combine their various parts, without much time, and repeated examination; the reduced size of prints lays the whole at once before the eye, and tends to facilitate not only comprehension, but contrivance. Whoever can delineate progressively as he invents, saves much labour, much time, and the hazard of confusion. Various contrivances have been employed to facilitate drawing in perspective, as may be seen in "Cabinet de Servier, Memoirs of the French Academy, Philosophical Transactions, and lately in the Repertory of Arts." The following is simple, cheap, and portable.

"Plate XVI. Fig. 1. A, B, C, represent three mahogany boards, two, four, and six inches long, and of the same breadth respectively, so as to double in the manner represented. Fig. 2, the part A is screwed, or clamped to a table at a convenient height, and a sheet of paper, one edge of which is put under the piece A, will be held fast to the table. The index P is to be set (at pleasure) with its sharp point to any part of an object which the eye sees through E the eye-piece.

"The machine is now to be doubled as in Fig. 2, taking care that the index is not disturbed; the point, which was before perpendicular, will then approach the paper horizontally, and the place to which it points on the paper must be marked with a pencil. The machine must be again unfolded, and another point of the object is to be ascertained in the same manner as before; the space between these points may be then connected with a line; fresh points should then be taken, marked with a pencil, and connected with a line; and so on successively till the whole object is delineated."

The

Method of using
a plain graduated
ruler for taking
views, &c.

The above machine affords a delineation which is strictly accurate: but I take this opportunity of mentioning one still more portable, though less exact, which may be used in taking small sketches in the field; where the table, and fixed sheet of paper cannot always be supposed at hand. I do not know the contriver. It is merely a strait flat ruler, having a division of inches and small parts (or any other division) on its edge. A string is fastened to the middle of the ruler by passing it through a hole, and tying a knot on the other side; and at the other end of the string there is a small bead or knot to be held in the mouth. The length of the string may be adjusted at pleasure; and when the ruler is used, it is held up at right angles to the stretched string, so that its edge, as seen by one eye, may apply to any two objects; between which it will shew the distance to be afterwards transferred upon the paper by a scale, or by estimate.

Farther observations.

In this use of a graduated rule, it is most convenient and accurate to select some one object in the picture for the point of sight, and to measure all the distances from thence from the middle or beginning of the divisions where the direction of the sight is at right angles to the rule. And as this simple instrument does not give the inclinations, it may be best always to measure parallel or perpendicular to the horizon, and estimate the rest. Indeed, the contrivance must be considered only as a substitute for the usual method of estimating, and may be principally useful to assist in acquiring a correct judgment in this respect.

W. N.

IX.

Concerning the new Planet CERES. In a Letter from the Rev. WILLIAM PEARSON, including an Extract of a Letter from the BARON DE ZACH to Mr. EDWARD TROUGHTON.

To Mr. NICHOLSON.

S I R,

Parson's Green, March 9, 1802.

IN the discovery of the new planet Ceres is an event which has already engaged the attention of the principal astronomers of Europe, and will continue to be a subject of discussion until

Introductory remarks. Popular account of the manner of applying observations.

until the elements of its orbit shall be accurately assigned, it may be acceptable to many readers of your Journal to have a popular detailed account laid before them of the manner of applying observations to determine the whole period and form of the orbit, in order that they may comprehend, and consequently feel an interest in, the perusal of the different notices which will, most probably, be published from time to time in different countries.

When a heavenly body is first suspected to be a planet, the suspicion arises either from its apparent aspect compared to a star when viewed in a telescope, as was the case with Geo. Sidus, or otherwise on account of an observed change of relative situation, as was the case with the new planet Ceres when compared with some small stars in its neighbourhood: the first thing to be done after the supposed discovery of a new planet is, to ascertain its right ascension and declination, and from thence its geocentric longitude and latitude; the means used for doing which need not be described here.

A series of observations, and corresponding calculations, are usually continued at successive intervals, until the newly discovered body has advanced or receded through such a portion of the ecliptic as to afford data for estimating its daily velocity when compared to that of the earth: now, if a series of observations could be made by an observer in the sun, the arc passed through by the planet would bear the same proportion to the interval of time elapsed between the first and last observations, that a circle does to the whole period, provided the motions were *equable*; and, if the motions were *unequable*, the observed progress at equal intervals would shew whether the inequalities were increments or decrements, and consequently whether the planet was approaching the perihelion or aphelion point. But the observed places are *geocentric*, and must therefore be converted into *heliocentric*, to gain those observations which an observer placed in the sun would make; for which purpose the proportion to be used will be, by a simple case in plane trigonometry, *as the distance of the planet (to be at first assumed) from the sun: is to the sine of its observed elongation, or angular distance from the sun (or its supplement): so is the earth's distance from the sun, taken from the tables, to the sine of an angle which is the difference between the heliocentric and geocentric places, and which is called the parallax of the orb.*

How a planet is discovered—by its aspect through the telescope;—or its motion.

Subsequent observations. These observations being made from the earth or geocentric, must be converted into heliocentric results, or such as would appear from the sun.

Manner of doing this.

From an heliocentric arc passed over in a known time, its *approximate period* is deduced;—and thence by Kepler's law a *correcter distance*. And the computations may thus be repeatedly amended.

Errors from eccentricity,

corrected by determining the elementary points of the orbit, &c.

Determination of the obliquity and place of the nodes.

The heliocentric arc, which the planet has passed through in a given time, being thus *nearly* ascertained, the whole *approximate period* is next found on a supposition of equal arcs being passed through in equal times; which will afford data for determining a somewhat more accurate distance of the planet from the sun, by the well known law of Kepler, by which the squares of the periodic times are analogous to the cubes of the mean distances of the earth and any other planet respectively: an *approximate* distance being thus obtained may, in the next place, be substituted for the *assumed* distance, and the parallax of the orb be determined a second, and even a third time in this way; by means of which repetitions an *approximate* period, and a corresponding *approximate* distance, will be obtained. These will vary more considerably from the truth the greater the distance of the observed arc is from one of those two points of the orbit where the planet has a mean motion; which points are always nearer to the aphelion than to the perihelion point, by a quantity which depends upon the eccentricity. If therefore it should so happen that a new planet, at the time of its first discovery, were at, or very near, its mean distance, the whole period and distance obtained from a few of the first observations would be pretty accurate.

The next step to be taken is to determine the increments or decrements of motion, when a number of geocentric are changed into heliocentric places, and thus to trace the points in the ecliptic where the velocity is a *maximum*, where it is a *minimum*, and where it is a *mean*. These will shew where the perihelion and aphelion points are, and also the place of equated anomaly at an instant when the equation is a maximum; these data afford the means of calculating the greatest equation and corresponding eccentricity; but before they can be assigned with accuracy, a considerable time must elapse to afford the astronomer an opportunity of observing a few successive oppositions or conjunctions and stationary points, for the purpose of correcting the approximate elements, and of determining the true shape and position of the orbit.

In the mean time the geocentric latitudes, gained by observation, must be also converted into heliocentric latitudes, in order to determine the nodes or points where the two opposite sides of the orbit cross the ecliptic. For doing this the analogy

is—*As the sine of the planet's elongation from the sun : — is to the angle of commutation (or difference between the helio. longitudes of the planet and of the earth)—So is the tangent of the geocentric lat : is to the tangent of the heliocentric latitude.*

From this account of the means necessary to be used in ascertaining the precise nature of the orbit of a newly discovered planet, it will be naturally inferred that the elements already assigned to the orbit of the new planet Ceres cannot be very accurate, allowing them to be duly proportioned to one another, because an error in any one of its elements renders all the rest erroneous ; and there has not yet elapsed, probably, much more than one-fourth of an entire period since its discovery, nor has it yet been in the most essential portions of its orbit for affording the best data. Hence a continuation of accurate reports concerning this planet ought to be publicly recorded from time to time, in order that a comparison of different and distant observations may afford the requisite data for ultimately settling its elements with accuracy. I shall therefore make no apology for laying before your readers an extract from a letter of Baron Von Zach to his friend and correspondent, Mr. Edward Troughton, mathematical instrument-maker, of Fleet Street, who has very obligingly put it into my hands, with permission to make what use of it I may think proper. The letter is unusually long, and full of interesting matter ; but I shall confine myself, in this communication at least, to those parts of it which principally relate to the new planet, and the subjects connected with it.

From the preceding account it follows that no elements of the new planet can yet be accurate.

Extract of a Letter from the Baron Von Zach to Mr. Edward Troughton. *Letter from Von Zach.*

Gotha, January 28, 1802.

“ ***** YOU have heard perhaps, dear friend, that I was so lucky as to discover again Mr. Piazzi's planet, called now, in honour of the king of Naples, *Ceres Ferdinandea*. I found this little planet first on the 7th of December last year, just between the head and the north wing of Virgo, in $178^{\circ} 33\frac{1}{2}'$ right ascension, and $11^{\circ} 41\frac{1}{2}'$ declination N. An astronomical friend of mine, Dr. Olbers, in Bremen, found

Re-discovery of the planet Ceres.

found the planet on the 2nd of January between the N. \circ 20 $\frac{1}{2}$ and ϵ Virginis. I have not heard that this little planet, which resembles a star of the ninth magnitude in size, has been seen in France and England. I have already given information of my discovery to the president of the Royal Society, Sir Joseph Banks. I have the honour to send you here my observations of this planet.

Table of observations.

SEEBERG OBSERVATORY.

	Mean Time.	Ap. Right Ascen.	Declination.
1801. 7 Dec.	18 ^h 48' 10,3"	178° 33' 30,6"	11° 41 $\frac{1}{2}$ ' N.
31	17 38 $\frac{1}{2}$:	184 44 :	11 5
1802. 11 Jan.	17 3 17,4	186 45 49,95	11 15
16	16 46 25,6	187 27 53,25	11 26
22	16 25 23,9	188 6 25,8	11 44
25	16 14 32,9	188 20 39,15	11 54
26	16 10 53,7	188 24 49,5	11 57

" If you, or your astronomical friends, wish to look at the new planet Ceres, which is rather difficult, this heavenly body being so small, I send you here a little ephemeris, which will direct you in the research of this planet. With my eight feet transit instrument, by the late Mr. Ramsden, and with 200 magnifying power, I could not perceive the least mark of a disk. Perhaps Mr. Herschel will see more: perhaps he will discover some satellites to Ceres.

Ephemeris.

Position of the Ceres Ferdinanda for Midnight, in the Seeberg Observatory.

	Right Asc.	Decl.	Rt. Asc. in Time
1802. Feb. 2	6 ^h 8 ^m 43"	12° 31'	12 34 53
	5 6 8 45	12 47	12 35 1
	8 6 8 44	13 4	12 34 55
	11 6 8 38	13 22	12 34 33
	14 6 8 29	13 41	12 33 57
	17 6 8 16	14 1	12 33 5
	20 6 8 0	14 21	12 31 59
	23 6 7 40	14 42	12 30 40
	26 6 7 17	15 3	12 29 8
March 1	6 6 50	15 24	12 27 22

Difficulties in the observations.

" How much I want your four feet circle in my observatory, my present perplexity will show you. Hitherto I have observed all my zenith distances with a four feet Dollond's quadrant,

quadrant, and a circle of two feet from Cary; the telescopes have little aperture, so that I cannot see the new planet Ceres, and consequently I cannot observe the zenith distances: as the air all January was very hazy and cold, I had much ado to see the planet in my very excellent transit instrument. I must therefore wait till the planet shall increase in brightness, which yet will not go farther than to a star of the seventh magnitude. In the mean time I hope that by very clear weather I may distinguish the planet in my old quadrant, or in my little circle, and that this faint body will support the illuminating of wires, which has not been the case hitherto. This is the reason that I could not obtain one exact declination, but my right ascensions of Ceres are extremely exact. This difficulty will certainly never take place in my observatory when I once get your four feet circle. * * * * *

"As soon as the instruments arrive, I shall give you notice of it: in the mean while I have the honour to subscribe myself, with the greatest regard and highest esteem, which the reigning Duke of Saxe Gotha very sincerely shares with me,

Most honoured Friend,

Your most obedient Servant,

And devoted Friend,

FRANCIS BARON DE ZACH,

*Lieutenant-Colonel, and Director of
Seeberg Observatory.*

"Before I finished this letter, I had two observations more of the new planet Ceres, viz. Latest observations.

		Rt. Asc. Ceres.	Decl. Ceres.
28 Jan. 16 ^h 3' 29"	M. T.	188° 31' 37,85"	12° 9' 41,3"
		tolerably well at the quadrant.	
29	15 59 43,7	188 34 18,13	12° 14'
		only guessed."	

This extract affords me an opportunity of illustrating the account, which has been just given, of the application of two distant observations of a planet for determining the heliocentric arc that has been run through in a given time: if I take the right ascension of Ceres for the 1st of Jan. 1801, as given by

Piazzi's first observation, and also for the 26th of Jan. 1802, as given by the last observation of Von Zach in the present extract, and project the orbits of the earth and Ceres according to the eccentricity, mean distance and place of the aphelion of Gauss, as given in the extract from the *Moniteur* in the different journals, we shall have the figure represented in Plate XIII. Fig. 1, which is thus constructed, viz.

Construction of the orbits of the earth and Ceres, with the geocentric and heliocentric places of the latter, &c.

Supposing the point S to be the sun, describe, with any radius, the circle marked φ , γ , π , &c. for the ecliptic, and conceive it to be at an *infinite distance*; draw an occult line from S to \odot $9\frac{1}{2}^\circ$, the earth's aphelion, and another to \approx $26\frac{1}{2}^\circ$, the aphelion of Ceres; take 10 from a scale of equal parts, and describe with that radius the innermost circle A \oplus P from a point $\frac{1}{20}$ of the radius from S towards A in the occult line, and another circle with a radius of 27,6 of those equal parts from the point C $\frac{1}{20}$ of the said radius from S in the second occult line, and these circles, which are eccentric with respect to the sun at S, will very nearly represent the required orbits of the earth and Ceres, in both which A represents the aphelion, and P the perihelion points. In the next place find the two points in the earth's orbit, which are diametrically opposite the sun's place for Jan. 1, 1801, at 9^h P. M. and Jan. 27, 1802, at 4^h A. M. which will be \odot $11^\circ 1' 33''$ and Ω $6^\circ 35'$ respectively, and mark them as in the figure with their dates; after this mark the geocentric right ascensions of Ceres (denoted by the letter C with a cross beneath) for Jan. 1, 1801, and G at γ $21^\circ 47' 48''$, according to Piazzi, and for Jan. 26, 1802, at G at \approx $8^\circ 21'$, according to Von Zach, and draw the two dotted lines from S to each; then if dotted lines be drawn parallel to these two lines from the earth (\oplus) in Jan. 1801 and 1802 respectively, until they touch the orbit of the planet Ceres, these last lines will mark the geocentric *apparent places* of this planet in its orbit, of which the dotted lines from S to the ecliptic denote the *measure*; and, lastly, if lines be drawn from S through the geocentric places to the ecliptic to H and H₁, they will indicate the heliocentric longitudes, and the arcs contained between the heliocentric and geocentric measures will be on each day the measure of what is called the parallax of the orb.—This method of converting geocentric into heliocentric places by projection is capable of great accuracy, and is, so far as I know, original.

The method of converting geocentric portion into heliocentric

The

The reason of drawing the occult *parallel* lines, which may not occur to the reader, is this; as the ecliptic is considered to be an infinite distance, so that the whole orbit of the earth, if seen from it, would appear only as a point, a line drawn to it from the sun, and another parallel thereto from the circumference of the earth's orbit, will, to an eye placed in the ecliptic, even if it were only at the distance of a star, appear *coincident* on the *same line*; hence if a line be drawn from the earth to the planet in its orbit, let their respective situations be what they may, another line drawn parallel to that from the sun, till it reaches the ecliptic, will shew the geocentric measure therein the same as if that measure were taken in a graduated ecliptic described from the earth as a center, and having all its divisions exactly parallel to those of the ecliptic described from the sun as a center.

Indeed, I have tried this projection with the geocentric and heliocentric places of some of the other planets taken from an ephemeris, and find it extremely accurate as well as easy.

by projection is considerably accurate;—and new.

Tried with the other other planets.

From this projection of the heliocentric and geocentric places of Ceres, it appears evident that the distance from the earth to it was much greater last summer when the earth was at P, than it is at present, and it is equally evident, that as the velocity of the earth is greater than that of the new planet, and as they were both moving in the same direction at the latter period in the projection, the distance will continue to diminish until both the planets are in the same strait line as seen from the sun, agreeably to what is said in the extract from Von Zach's letter; for the apparent magnitude is in an inverse ratio of the distance, so that as the distance diminishes, the apparent disk increases. The reason also appears clear, why there was very little apparent motion of the new planet in the last month as seen from the earth, for in this part of the earth's orbit Ceres would *appear* to have a retrograde motion, provided it were at *rest*, but its forward motion in a certain degree balanced that, and produced the effect of little or no apparent motion at all:—on the 6th or 7th of Feb. last it was stationary, and has been since retrograde in a small degree.

Remarks from the projection of the relative motions and apparent magnitude of the planet.

termination of
heliocentric
passed
rough,

In order to ascertain by calculation the heliocentric arc passed through in 390,3 days, the time contained between Jan. 1, 1801, at 9 P. M. and Jan. 27, 1802, at 4 A. M. the heliocentric longitudes of the new planet may be ascertained thus;

	S	°	'	"
1801, Jan. 1 ^d 9 ^h sun's long.	9	11	1	33
Right ascension of Ceres	1	21	47	48
Elongation	7	19	13	45

Then at 27 (supposed distance) 1,43136
Is to sine of 49° 14' 9,87931
So is 10 (earth's distance) 1,00000

10,87931
1,43136

To sine of paral. of orb. 16° 17' 9,44795

	S	°	'	"
Again 1802, Jan. 26 ^d 16 ^h sun's long.	10	6	35	
Right ascension of Ceres	6	8	24	
Elongation	3	28	11	

Then as 27 1,43136
Sine of 61° 49' (supplement) 9,94519
So is 10 1,00000

10,94519
1,43136

To sine of paral. of orb. 19° 3' 9,51383

Geocentric place of Ceres.	S	°	'	"	
Jan. 1, 1801	1	21	47	48	Parallax 16 17
Jan. 26, 1802	6	8	24	49	Ditto 19 3
Whole geocentric arc	4	16	37	1	35 20
Deduct sum of parallaxes 1 5 20					

Whole heliocentric arc 3 11 17 1 in 390,3 days.

thence of
period
7,27 days,

Now, if the motion of the planet were equable throughout its orbit, we should have the whole tropical period from this ratio, as 101° 17' 1" : 390,3^d :: 360° : 1387,27 days, but the portion

portion of a circle, which has been past since Jan. 1, 1801, is that in which the velocity has been above a mean velocity; therefore the period thus obtained is shorter than the true period by a quantity which depends upon the equation of the center, which has not yet been examined. At present want of leisure, and the length of this paper, render it necessary that I should defer entering upon the other particulars, which remain to be discussed, some of which, it is presumed, will be found interesting; for I propose to show that some of the elements assigned by Gauss, which are considered as the most accurate, are not in due proportion to one another. This discussion will therefore constitute the subject of another communication. In the mean time we may infer, either that the whole period is shorter than has been prematurely determined, or otherwise that the greatest equation should be much greater than the eccentricity at present assigned requires. Indeed, it will hereafter appear that the eccentricity, given from the *Moniteur* in the different journals, is almost *two-thirds* too little to correspond to the equation which has been attributed to the new planet.

which, for reasons given, is too short,

though the period generally given must either be too long; or the greatest equation must exceed what the assigned eccentricity requires.

I am, SIR, as usual,

Your's very respectfully,

W. PEARSON.

The early observations of Professor Piazz, to which Mr. Pearson refers, are given by the professor in the following tables. The computations in the latter table were made from elements which, at the present time, would require to be greatly amended; but I have not chosen to omit them. W. N.

Early observations of Piazz,

Table of the Mean Time, Right Ascension, and Declination of the new Star as observed; together with the Longitude of the Sun, and the Logarithm of its Distance from the Earth.

Right Ascensions and Declinations of Ceres in 1801.

Days of the Month.	Ten Thousandths of the day in mean Time.	Right Ascension.	Declination.	Sun's Place.	Log. Distance of from ☉
					⊕
1	3635	51° 47' 48.7''	15° 37' 43.5'' N	9° 11' 1' 33.1''	9.992617
2	3606	43 27.7	41 5.5	12 2 31.7	9.992629
3	3577	39 36.1	44 31.6	13 3 30.2	9.992641
4	3547	35 47.2	47 57.6	14 4 29.0	9.992652
10	3378	23 1.5	16 10 32.0	20 10 29.5	9.992768
11	3350	22 26.0	14 30.4 est.	21 11 29.5	9.992794
13	3295	22 34.5	22 49.5	23 14 28.0	9.992848
14	3268	22 55.8	27 5.7	24 14 27.3	9.992882
17	—	27 35.1	40 13.0	—	—
18	—	28 45.1	—	—	—
19	3136	32 2.2	49 16.1	29 19 14.1	9.993060
21	3084	38 34.0	58 35.9	10 1 21 2.5	9.993151
22	3059	42 21.3	17 3 18.5	2 21 55.1	9.993196
23	3033	46 43.5	8 5.5	3 22 46.4	9.993242
24	—	51 45.1	—	—	—
28	2909	52 13 38.3	32 54.1	8 26 45.5	9.993522
30	2860	27 2.1	43 11.0	10 28 10.6	9.993645
31	2837	34 18.8	48 21.5	11 28 55.5	9.993708
January.					
1	2813	41 48.0	53 36.5	12 29 36.6	9.993773
2	2789	49 45.9	58 57.5	13 30 17.0	9.993851
4	—	53 7 45.1	—	—	—
5	2719	15 40.5	18 15 1.0	16 32 13.9	9.994083
8	2650	44 37.5	31 23.2	19 35 2.2	9.994328
11	2583	54 16 23.1	47 58.8 N	22 35 41.3	9.994588
February.					

N. B. The observations marked with two points (:) are a little doubtful; those marked with (:) very uncertain.

Table of the Geocentric Longitudes and Latitudes of the new Star, both by Observation and Calculation; together with their Differences,

Days of the Month.	Geocentric Longitude.		Differ.	Geocentric Latitude.		Differ.	Longitudes and latitudes of Ceres, Jan. Feb. 1801.
	Observat.	Calculat.		Observat.	Calculat.		
1	1° 23' 22" 58.5"	1° 23' 21' 59.2"	-59.3"	3° 6' 32.4"	3° 6' 50.2"	+17.8"	
2	19 44.8	18 40.2	-64.6	2 13.1	2 27.7	+16.6	
3	16 49.3	15 47.1	-62.2	2 57 58.9	2 58 63.3	+7.4	
4	14 16.5	13 18.9	-57.6	53 44.5	53 48.4	+3.9	
10	7 59.4	7 19.5	-39.9	28 50.9	28 31.8	-19.1	
11	8 25.7	7 43.4	-42.3				
13	9 58.0	9 38.9	-19.1	16 49.0	16 21.0	-28.0	
14	12 1.6	11 32.9	-29.0	12 47.1	12 23.2	-23.9	
19	25 49.4	25 51.5	+2.1	1 53 28.3	53 1.3	-27.0	
21	34 21.8	34 23.4	+1.6	45 58.9	45 31.6	-27.3	
22	39 1.8	39 6.7	+4.9	42 18.7	41 51.3	-27.4	
23	44 15.6	44 17.4	+1.8	38 39.2	38 12.3	-26.9	
28	24 15 16.0	24 15 28.1	+12.1	20 58.7	20 32.0	-26.7	
30	30 5.4	30 23.0	-17.6	14 5.3	13 43.5	-21.8	
31	38 8.6	38 20.5	-11.9	10 45.0	10 22.4	-22.6	
1	46 19.6	46 38.0	+18.4	7 23.8	7 3.6	-20.2	
2	54 55.6	55 12.8	+17.2	4 0.7	3 47.0	-13.7	
5	25 22 43.5	25 22 52.8	+9.3	0 54 19.0	0 54 9.6	-9.3	
8	53 17.9	53 15.6	-2.3	44 42.7	44 50.9	+8.2	
11	26 26 26.1	26 26 24.8	-1.3	35 47.9	35 50.4	+2.5	

X.

Entertaining chemical Experiments; with Notices of various new Facts and Observations respecting the Products of Nature and Art. By Mr. FREDERICK ACCUM. Communicated by the Author.

1. Production of phosphorated Hydrogen Gas*.

AS the usual method of obtaining phosphorated hydrogen gas, and exhibiting its spontaneous inflammation, requires a considerable share of attention and manual dexterity, the amateur of chemistry will deem it not superfluous to have no-

* This and the five following Experiments were first noticed by Mr. Davy, who exhibited them in his lectures at the Royal Institution.

ticed

ticed here the method of producing this gas in a more easy, expeditious, and economical manner, by merely presenting phosphorus to nascent hydrogen.

Zinc, sulphuric acid, and phosphorus, afford the gas;

which takes fire spontaneously, and continues to burn in a beautiful manner.

For this purpose, let water be decomposed in the usual manner, by means of zinc and sulphuric acid, and add to the mixture a quantity of phosphorus. The hydrogen evolved will dissolve part of the phosphorus; phosphorated hydrogen gas will be produced, and take fire at the surface of the fluid, so long as the decomposition of the water is made with considerable rapidity. But the gas produced in this process burns with a more lambent flame than that obtained in the usual manner, probably on account of containing a larger quantity of hydrogen. The experiment is nevertheless brilliant; for the gas is disengaged in small bubbles, which cover the whole surface of the fluid; they disengage themselves rapidly, new ones are produced, and the whole fluid resembles a well of fire.

Particular instructions.

For the success of this experiment, it is essential that the water, during the action of its decomposition, be considerably heated, which may be effected by a copious addition of sulphuric acid, and that the phosphorus be present in a considerable quantity. Half a part of phosphorus cut into small pieces, one of granulated zinc, three of concentrated sulphuric acid, and five of water, answer this purpose exceedingly well.

2. Phosphorated Hydrogen Gas burns with a green Light in nascent hyperoxigenised Muriatic Acid Gas, under the Surface of Water.

Phosphuret of lime, hyperoxigenised muriate of potash, phosphorus, and sulphuric acid, produce fire under water.

Put into an ale-glass, or Florence flask, one part of phosphuret of lime, broken into pieces of the size of a pea [not in small fragments or in powder], and add to it half a part of hyperoxigenised muriate of potash. Fill the vessel with water, and bring carefully into contact with the materials at the bottom of the fluid, three or four parts of concentrated sulphuric acid. This may most conveniently be done, by letting the acid fall through a long-necked funnel, reaching to the bottom of the vessel, or by causing it to pass down the sides of it. As soon as the decomposition of the water, and that of the hyperoxigenised muriate, takes place, flashes of fire dart from the surface of the fluid, and the phosphuret illuminates the bottom of the vessel with a beautiful green light.

3. Combustion

3. *Combustion of phosphorated Hydrogen Gas, by the Admixture of hyperoxigenised Muriatic Acid Gas.*

If phosphorated hydrogen and hyperoxigenised muriatic acid gas be mixed together, fire and flame are produced; and simple muriatic acid, water, and phosphoric acid, are formed. Flame by mixture of two gases.

4. *Combustion of inflammable Substances in nascent hyperoxigenised Muriatic Acid Gas.*

Put into a wine-glass one part of hyperoxigenised muriate of potash perfectly dry; pour on it, two of colourless sulphuric acid of commerce: a violent action will take place, and hyperoxigenised muriatic gas becomes evolved. If, during the extrication of this gas, one part of sulphuric ether, alcohol, or oil of turpentine, be suffered to fall into the gas, an accension takes place, accompanied with a crackling noise. Numerous combustions in hyperoxigenised muriatic gas:

In this manner not only all the inflammable fluid bodies, but likewise most of the solids, such as camphor, resin, tallow, pitch, elastic gum, &c. may easily be inflamed.

5. *Combustion of expressed Oils at the Surface of Water, by Means of hyperoxigenised Muriatic Acid Gas.*

Put into an ale-glass one part of hyperoxigenised muriate of potash; add to it three or four of water, and half a part of oil of olives, or of linseed. On adding to it four or five parts of concentrated sulphuric acid, a violent action takes place, much charcoal becomes deposited, and a multitude of ignited sparks pass through the black fluid, exhibiting a beautiful phenomenon. On adding an additional quantity of hyperoxigenised muriate of potash and sulphuric acid, the whole mass takes fire, and burns with a dense yellow flame. of oils on water;

6. *Combustion of Phosphorus in hyperoxigenised Muriatic Acid Gas, under the Surface of Water.*

Let fall into a wine-glass, or rather into a long cylinder, filled two thirds with water, one part of phosphorus, and two of hyperoxigenised muriate of potash. On adding to this mixture three or four parts of sulphuric or nitric acid, the phosphorus takes fire, and burns vividly under the surface of the fluid: on agitating the mixture, streams of ignited sparks pass through the fluid rapidly. phosphorus under water.

Remark.

Remark.—This and the two preceding experiments require caution. The operator ought to be distant during the affusion of the acids, or the addition of the combustible body, which are sometimes thrown out of the vessel to a considerable distance.

7. *Decomposition of sulphureous Acid Gas, by Muriate of Tin.*

Decomposition
of sulphuric gas
by mur. of tin.

If sulphureous acid gas, and fresh prepared muriate of tin, be brought into contact, the volume of the gas becomes speedily diminished, sulphur is deposited, and the simple muriate becomes converted into an oxygenated muriate of tin.

8. *Syrup of Violets, which has lost its Colour, regains it, when agitated in Contact with Oxygen Gas.*

Syrup of violets
restored by oxygen.

It is a fact well known to chemists, that syrup of violets is apt to lose its colour by age, though not the least perceptible change in the saccharine solution has taken place, when exposed to light, or even when frequently agitated in contact with atmospheric air. In order to make the discoloured syrup regain its primitive blue colour, nothing more is necessary than to agitate it for a few minutes in contact with oxygen gas.

9. *Phosphorated Hydrogen Gas is decomposed by Light.*

Light decomposes
phosphorated
hydrogen.

Though phosphorated hydrogen gas may be kept over mercury in the dark for any length of time unaltered, this is not the case if the gas be exposed to light. In this situation the union of the phosphorus and hydrogen is broken, the phosphorus becomes separated, and crystallizes in the vessel, and the hydrogen is left behind.

10. *Acetite of Barites is capable of crystallizing.*

Crystals of acetite
of barites.

A solution of acetite of barites, prepared by dissolving pure barites in pure acetic acid, which had been exposed to light in an open glass vessel, was found to be converted into a solid regular crystalline mass. The form of the crystals could not be determined, on account of their extreme minuteness.

11. *Spontaneous Reduction of Howard's fulminating Mercury.*

Spontaneous reduction
of fulminating
mercury.

Four ounces of Howard's fulminating mercury were placed still wet on a chalk stone, exposed to the rays of the sun in a window. It was left in this situation unobserved for at least
three

three months. The product during this time was converted into a black brilliant powder. On attempting to collect it together into one heap, and separating it from the paper which had been interposed, a globule of running mercury was seen. On introducing the powder into a bottle, and shaking it together, heat was evolved, and the whole mass became reduced to the metallic state.

12. *Production of red Sulphuret of Mercury in the humid Way.*

When equal quantities of a concentrated solution of oxygenated muriate of mercury, and concentrated fresh prepared fuming hydrosulphuret of ammonia, are mingled together, a brownish muddy precipitate is produced, which, when left undisturbed, turns yellow in three or four days, then orange, and at last acquires a beautiful cinnabar-red colour. The sulphuret of mercury, when separated, possesses all the properties of the best vermilion which is met with in commerce.

Cinnabar in the humid way.

13. *Air Bladder of the Carp contains common Air.*

The air bladder of the carp does not contain nitrogen, as Fourcroy, &c. assert. If the air of the bladder of this living fish be examined, it will be found to contain atmospheric air, and not nitrogen.

Error of Fourcroy concerning azote.

14. *Fish Bones contain much phosphoric Acid.*

The bones of fish contain upwards of one sixth part more of phosphoric acid, than those of quadrupeds. They may therefore advantageously be employed for making phosphorus.

Fish bones abound with phosphorus.

15. *Wax from Spiders.*

The yellow matter deposited in vessels containing spiders preserved in alcohol, is a true wax, which may be obtained from these animals by gently heating them.

Spiders' wax.

16. *Fluoric Acid dissolves Lead.—Method of obtaining it pure.*

Fluoric acid obtained in the usual manner, by decomposing fluuate of lime by sulphuric acid in a leaden retort, always contains lead in solution; as may be proved by mingling the acid with a solution of water impregnated with sulphurated hydrogen gas. Tin vessels are less acted upon than those made of lead. The acid has no effect upon silver.

Common fluoric acid contains lead.

In

How to obtain it
pure.

In order to obtain pure fluoric acid, one part of fluoric acid, reduced to a fine powder, may be mixed with two of sulphuric acid, and one of water. The mixture is then to be introduced into a glass retort, to which a glass receiver has been previously luted, containing two ounces of water. Heat being then applied, the distillation is to be carried on slowly. After the decomposition of the fluoate of lime is effected, which may be known by the disappearance of the whitish vapour in the retort, the contents of the receiver must be filtered, and distilled water added to the filtered fluid, till, on a new admixture of water, no further cloudiness appears. The acid thus obtained contains no flux; it is absolutely pure; and may be kept in glass bottles covered within with wax, or, in preference, with a hard varnish.

17. Benzoic Acid exists in Vanelloe.

Crystalline matter
on vanelloe.

A quantity of vanelloe pods [*Epidendrum Vanilla*, L.] which had been kept wrapt up into a bladder, and surrounded with thin sheet lead for upwards of four years, were found to be covered with a white powder of a pungent saline taste. On examining the internal parts of the wrinkled shell of the pods, the black seeds, when viewed under the magnifier, were found to be covered over with a multitude of oblong crystals, crossing each other in all directions.

Experiments
proving it to be
benzoic acid.

In order to examine these crystals, one ounce of the pods were broken into small pieces, and boiled in a Florence flask, with a quarter of an ounce of lime and six ounces of distilled water, for about ten minutes. The whole mass was then suffered to repose, and the clear fluid, which was of an amber colour, and aromatic bitter taste, was set aside. The residue was treated in a similar manner, and the fluid was added to the first. The clear solutions were then slowly evaporated to one fourth. During this process a quantity of a brown adhesive substance separated, which had all the properties of a true resin; its weight amounted to nine grains. On letting fall into the concentrated solution, from which this resin had been separated, some pure muriatic acid, the whole became turbid, and a pulverulent yellow precipitate was deposited, which increased in quantity on heating the mixture. All the precipitate obtained in this manner was then dissolved in distilled water, filtered, evaporated, and crystallized; it yielded beautiful

ful lemon-yellow crystals, which could not be obtained colourless, by repeated solutions and re-crystallizations. But after boiling them with charcoal powder, the fluid passed colourless through the filter, and yielded pure benzoic acid in the form of silky needles, weighing 23 grains.

18. *Camphor does not move upon Water at low Temperatures—
Phosphorus moves upon Mercury.*

If a basin of water, upon which small fragments of camphor are in rapid motion, be suddenly transferred into a freezing mixture, the rotatory motion of the atoms of camphor instantly ceases. Camphor motionless on water in a very cold atmosphere.

If a cylinder of dry phosphorus be scraped with a knife over a saucer containing mercury free from dust, &c. the small particles of phosphorus which are detached, and fall upon the quicksilver, spin in a similar manner like those of camphor placed upon clean water. Phosphorus moves on mercury;

Camphor, benzoic acid, musk, castor, civet, assafœtida, vanillœe, and various other odoriferous substances, remain motionless when placed upon this fluid. but camphor, &c. do not.

19. *Frogs change their Colour, and decrease in Size, when secluded from Light, and die when suddenly exposed to vivid Light.*

A number of male frogs of the largest size, and of a beautiful yellowish brown colour, which had been kept for upwards of six months in a reservoir, situated in a dark place, lost their colour during that time, became perfectly black, and had decreased in size to less than one half. A number of these animals, which were very brisk, and apparently in a good state of health, were exposed to the rays of the sun, in order to see if they would regain their colour. They all died, during the experiment, in about three hours. The galvanic influence could not stimulate their dead muscles into action. Frogs become black and smaller by keeping in the dark.
Light kills them
and destroys all irritability.

A frog, which had escaped, was found in a vessel containing a concentrated solution of potash, freed from carbonic acid, in which, from the time of discovery, he could not have remained longer than three hours. The animal was much distorted, and converted into a flesh-coloured gelatinous mass. It was beautifully transparent, and twice its natural size. On pouring boiling water upon it, the whole was dissolved into a jelly. A frog converted by potash into a soluble clear mass.

Habitudes of these animals in different fluids and gases.

Frogs kept in distilled water soon become languid, and die. They cannot live in water impregnated with nitrous oxide, nor in water holding carbonic acid gas in solution in considerable quantity. They live in an atmosphere of pure nitrogen for five or six days, if water be present. They instantly die in all hidrocarbonates; but may be kept alive in nitrous gas for some days.

XI.

The Method of crystallizing Lime. By TRAUMSDORFF.*

History of the crystallization of lime.

THE crystallization of lime was first discovered accidentally by Schaub. Bucholz also obtained very fine crystals of this earth by boiling it with its muriate. I have verified this discovery; and find that lime may be obtained in crystals equally well in winter as in summer; with the exception, that the salt prepared during the winter season forms crystals which are thicker and larger. In order to obtain these crystals, any quantity, at pleasure, of the muriate of lime is to be boiled with one fourth, or even less, of caustic lime, and the fluid concentrated until in winter a drop of the solution, let fall upon a cold stone, shall acquire the consistence of syrup without crystallizing or congealing. The fluid is then to be strained through a close cloth filter into a capsule of porcelain or earth, which is then to be covered with a similar capsule, or a wooden cover, in order that the cooling may be as slow as possible. By this means very long but slender crystals of caustic lime are obtained, which must be washed in alcohol to clear off the adherent muriate. This operation must not be attempted with a less quantity than seven pounds of muriate of lime.

Residue, after distilling ammonia from much lime, is the crystallized earth.

It is well known, that when muriate of ammonia is distilled with excess of lime, for its decomposition, part of the residue adheres so strongly to the retort, that it is almost impossible to detach it by softening it. This hard mass is for the most part formed of lime confusedly crystallized, which is more difficultly diffused in water than the same earth in powder, or in state of minute division.

* Journal der Pharmacie, vol. ix. part 1. p. 108.

XII.

*On the Crystallization of the Hydrosulphuret of Soda. By CIR. VAUQUELIN *.*

BERTHOLLET, in a memoir which he communicated to the Institute about four years ago, shewed that sulphurated hydrogen has several properties in common with the acids, such as that of reddening the tincture of turnsole, uniting with the earths, the alkalis, and the metallic oxides, and forming crystallizable combinations with some of these substances.

I have had occasion to observe, a few days ago, one of the combinations of this kind, namely, that of sulphurated hydrogen with soda. Having lixiviated a considerable quantity of soda, manufactured by Citizens Payen and Bourlier, to extract the carbonate of soda, I left the concentrated mother water in a corner of the laboratory. At the end of some decads, I found at the bottom of this liquor a white transparent salt, crystallized in rectangular tetrahedral prisms, terminated by four-sided pyramids, some of which were octahedral. As this is not the form assumed by carbonate of soda, I made some experiments to determine its nature. Its taste was at first of an acid, caustic and caustic, nearly resembling that of the alkalis, which led me to suspect that it might be caustic soda; but I was soon undeceived, by an insupportably bitter taste which succeeded, and by a slight odour of sulphurated hydrogen gas.

It is abundantly soluble in water; and notwithstanding the causticity, which seems to announce that the alkali is united with a slight acid, nevertheless it absorbed caloric in its solution. Its solution had no colour, but the smell of sulphur was stronger than that of the salt itself; the acids produced a lively effervescence, and developed a very strong smell of sulphurated hydrogen gas. But the fluid did not become turbid: the nitric and oxigenated muriatic acids, on the contrary, formed an abundant precipitate, which, when washed and dried, exhibited all the characters of slightly hydrogenated sulphur. The salt, or its solution, spread on blotting paper, soon assumed a dark green colour. Lastly, this salt precipitates all the metallic oxides from their solutions, perfectly

Berthollet's observations, that sulphurated hydrogen performs the functions of an acid.

Sulphurated hydrogen forms with soda a crystallizable salt;

of an acid, caustic taste;

which succeeded by extreme bitterness;

very soluble in water, and producing heat;

an effervescing with acids;

precipitated by the nitric and ox. mur. acids;

dark green as it dries;

precipitates metals like an hydrosulphuret;

* Annales de Chimie, No. 122, vol. xli. p. 190.

but does not throw down the earths; whence the alkali is saturated. similar to those which the same substances assume when precipitated by the artificial hidrosulphuret of soda. It does not precipitate the earths when dissolved in the acids, excepting alumine, zircon, and yttria; which proves that the alkali is perfectly saturated with sulphurated hydrogen gas.

With sulphate of iron it affords hidrosulphuret of iron, and sulphate of soda. This salt, when decomposed by sulphate of iron, afforded hidrosulphuret of iron, and sulphate of soda, which were obtained crystallized by evaporation and cooling of the liquor; whence this salt is a true hidrosulphuret of soda.

The manufacturers did not use chalk enough, &c. The presence of this salt in the soda of Messrs. Payen and Bourlier, proves that these manufacturers did not use a sufficiently large quantity of chalk to saturate the whole of the sulphur arising from the decomposition of the sulphate of soda by charcoal; unless we suppose that it is formed during the decoction the alkali is made to undergo when extracted from the carbonate of soda.

Remark. The whole novelty of this observation consists in the property which this salt has of crystallizing, and the determination of its figure. Its other properties have been described by Citizen Berthollet with great exactness.

XIII.

Memoir respecting a new Combination discovered in Zaffre, which CIT. BRUGNATELLI supposed to be a peculiar Acid, which he denominated the Cobaltic Acid. By CIT. DARRACQ.*

CITIZEN Brugnatelli, in a memoir printed in the *Annales de Chimie* for the month of Pluviose in the year 8, page 113 †, relates various experiments upon zaffre, or the grey oxide of cobalt, from which he concluded that this substance contains a new acid. I shall describe some of these experiments, as well as certain properties attributed to his cobaltic acid.

He digested zaffre in ammonia; After some days maceration in the sun he obtained a fluid of a red colour, known by the name of ammoniuret of cobalt, which he filtered and evaporated to dryness. The concrete residue thus obtained appeared to him to consist of two very distinct substances, one of a deep red colour, and the other of a pale

* *Annales de Chimie*, No. 121. Vol. XLI. p. 66.

† *Philos. Journal*, quarto, IV. 35.

a pale yellowish tinge. The red part was soluble in water, and the yellowish remained upon the filter. This residue he considered as the pure oxide of cobalt. The soluble part was evaporated, and by cooling it deposited certain small crystals, which Citizen Brugnatelli inferred to be a combination of the new cobaltic acid with ammonia. The supernatant liquor also possessed evident characters of acidity.

The author observed, that his acid might be obtained either coloured or colourless, according to the methods used in procuring it. Evaporations made by fire left a residue soluble in water, and afforded a cobaltic acid nearly colourless, whereas those made in the light of the sun always afforded it more or less red. Citizen Brugnatelli being desirous of ascertaining whether his acid was formed during the operation here described, or whether it existed ready formed in zaffre, boiled for twenty-four hours six pounds of this substance in eight pounds of water. He filtered the liquid while hot and evaporated it. When it was reduced to half it became turbid; the evaporation was continued until no more than one third of the fluid remained. It was then taken from the fire and deposited a white substance, which was collected upon the filter. The fluid which passed was of a bright yellow colour, with a sensibly acid taste; it acted in all respects the same as the cobaltic acid obtained by the process before pointed out.

It is either coloured or colourless.

Zaffre boiled with water afforded it.

The following are among the properties which Citizen Brugnatelli considers as characteristic.

Properties stated as characteristic of this acid.

1. It precipitates the solution of silver.
2. It precipitates lime water in a white coagulum insoluble in water, and in an excess of acid.
3. It is separated from its aqueous solution by alcohol.
4. It precipitates the acetite and muriate of barites.

I shall now proceed to describe the experiments I have made on this subject, and present to the Institute the consequences which I deduce from the same.

New experiments.

Experiment 1. I formed the ammoniuret of cobalt by leaving zaffre in digestion in the sun's light in ammonia. I was careful to agitate the mixture which was included in a matrafs, and soon obtained a red colour, that after forty-eight hours became a bright red. I observed in this fluid a crystallization in considerable abundance, in the form of white brilliant needles. This crystallization was permanent till the sun

Experiment 1.
The ammoniuret was formed.

The ammonia was evaporated by heat.

A precipitate fell and the filtered fluid afforded crystals by evaporation:

Part of the residue was taken up by water.

It was acid,

but it was not cobaltic acid.

Exp. II. The ammoniuret of cobalt was exposed to the sun to lose its ammonia.

Flocculent precipitate.

The clear fluid filtered and evaporated to dryness left a matter soluble in water, which was acid, and the same as in Exp. I.

The first residue on the filtre was arseniate of cobalt.

Exp. III. Powdered saffre was

had again heated the fluid, when it became dissolved in the liquid. In this state the ammoniuret of cobalt was filtered, and put into a retort; in proportion as its temperature increased it assumed a violet red colour which grew deeper and deeper, and at length had the appearance of the bright colour of red wine. When the greatest part of the ammonia was evaporated the mixture acquired a greenish colour, and by repose a substance of the same colour as the solution was precipitated. The liquid filtered while hot and evaporated to dryness, afforded rudiments of crystals, of which the form could not be determined. White and brilliant parts were observed, and the rest of the matter was of a yellowish colour. Upon this residue I poured water, and agitated it with a spatula of platina. The least coloured portion was totally dissolved, and communicated a straw colour to the water. This fluid was acid, and possessed some of the properties announced by Cit. Brugnatelli. I shall presently describe the experiments to which I subjected it, the results of which properly examined, prove that it is not an acid formed by the cobalt. The two residues obtained in this experiment, one of a greenish, and the other of a yellowish colour, were proved to be not pure oxide of cobalt, as Citizen Brugnatelli affirmed, but a combination of that oxide with the arsenic acid.

Experiment 2. Having prepared a new quantity of ammoniuret of cobalt as in the former experiment, I submitted to spontaneous evaporation in a situation where the sun accelerated the volatilization of the ammonia. In proportion as the liquor evaporated a flocculent matter of a whitish rose colour fell down, which sensibly increased until the ammonia was evaporated. The fluid emitted no smell, but it had preserved a rose colour of a considerable beauty. I filtered it to separate the precipitate, and again evaporated the liquor to dryness. The remaining matter was dissolved in distilled water. This solution of a light rose colour was acid, and possessed the properties of the acid obtained in the former experiment.

The residue upon the filter was carefully examined, and ascertained to be the arseniate of cobalt.

Experiment 3. I took, as the author of the memoir himself did, one kilogram of saffre, which was pulverised till it became an impalpable powder; I boiled it with three litres of distilled water for half an hour, and filtered the liquid while hot.

hot. The filtered fluid had a light colour, and perceptible taste. I evaporated it in a capsule of porcelain, and towards the end of the evaporation it became turbid. I continued to evaporate until the fluid was reduced to about one hectogram by cooling, and obtained needle formed crystals. The mixture was filtered with agitation; the crystals remained upon the filter, and the fluid passed very transparent of a bright yellow colour. Citizen Brugnatelli in making this experiment has not remarked the crystallization; he has only observed a white matter which was converted to a rose colour by the contact of the air, and supposed by him to be the oxide of cobalt. The needle formed crystallization which I obtained afforded a considerable disengagement of arsenic as soon as it was heated. When carefully examined it was proved to be the arseniate of cobalt, and not the oxide as had been announced. This fact is no doubt of little consequence to the discoveries of Citizen Brugnatelli; but nevertheless it might serve to support my opinion, if the results of my experiments were not more than sufficient to prove that the cobaltic acid has no existence.

boiled in water and filtered.

Crystals obtained by evaporation.

It gave out arsenic by heat; and was arseniate of cobalt.

The liquids of the 1st, 2d, and 3d experiments, which proved to be acid, and of the same nature, were subjected to the following trials, and comparatively with the arsenic acid.

Examination of the acid liquors.

First trial. This acid liquor is precipitated by sulphurated hydrogen, and by the alkaline hidro-sulphurets of a yellow colour similar to opiment, or the sulphuret of arsenic. This precipitate is in fact sulphuret of arsenic, and not sulphur precipitated, as Citizen Brugnatelli has supposed.

Sulphurated hydrogen throws down sulphuret of arsenic.

Second trial. It precipitates the ammoniuret of copper of a blueish green colour. This property belongs to the arsenic acid. The combination is known by the name of arseniate of copper.

It forms arseniate of copper with the ammoniuret of that metal.

3. Sulphate of copper mixed with this fluid affords a precipitate of the same colour as the ammoniuret of this metal. The arsenous acid also possesses this property not so eminently as the pretended cobaltic acid, but a similar precipitate may be obtained by using the arseniate of cobalt formed artificially. In this case the results are the same, and no difference can be perceived.

Its effect on sulphate of copper is the same as that of the arseniate of cobalt.

4. It precipitates the nitrate of silver of a white colour. The arsenic acid also enjoys this property.

It also acts like arseniate of cobalt on nitrate

and of mercury; 5. The nitrate of mercury is thrown down of a straw colour; and this phenomenon is also presented by the arsenic acid.

and lime water; 6. It precipitates lime water in a white coagulum insoluble in water, but not insoluble in an excess of acid; as Citizen Brugnatelli affirms; for in fact, it is re-dissolved with the same facility in the pretended cobaltic acid as in arsenic acid. I think the mistake of Citizen Brugnatelli arises from his not having used a sufficient quantity of acid.

and acetite and muriate of barites; from impurity. 7. It precipitates the acetite and the muriate of barites. I am assured that this precipitation arose from a small portion of sulphuric acid contained in the acid called cobaltic. The arseniate of cobalt also renders these solutions cloudy.

Precipitates in fusion of galls. 8. It forms an abundant yellowish precipitate with the tincture of nut gall newly made. It is known that the arsenic acid produces the same phenomenon.

9. The last trial. This last experiment appears to have been considered by Citizen Brugnatelli as the most characteristic of his cobaltic acid. He found that alcohol separated his acid from its aqueous solution, and also that the arsenic acid dissolved in alcohol enjoys the same property; by these processes he obtained his concrete acid. This property of the cobaltic acid at first surprised me; but presuming that I should discover the explanation by examining the acid thus precipitated, I subjected it to the following experiments.

The pretended cobaltic acid is separable from water by alcohol, and even by alcoholic solution of arsenic acid.

This precipitated acid gave out arsenic by heat; rendered borax violet;

The precipitating alcohol has acquired arsenic acid.

The acid precipitate was nearly insoluble in water;

but became soluble by addition of arsenic acid.

Hence the acid of Brugnatelli was arseniate of cobalt dissolved

1. When heated upon charcoal by the blow pipe it emitted white vapours very easily distinguishable to be those of arsenic.

2. A small portion heated with borax communicated a violet colour.

3. This acid being precipitated by alcohol alone I examined the liquid, after having separated it by the filter. Sulphurated hydrogen produced an abundant yellow precipitate, which was found to be a sulphuret of arsenic.

4. The separated acid was scarcely at all soluble in water.

5. When mixed with a few drops of arsenic acid it dissolved completely, and resumed its original properties.

From these trials I concluded, that the acid of Citizen Brugnatelli could be nothing else but the arseniate of cobalt dissolved in an excess of acid.

To put this beyond doubt I dissolved pure oxide of cobalt in the arsenic acid; I evaporated the solution to dryness, and added

added distilled water to the residue. After agitating the mixture it was filtered. The liquid thus obtained had a slight rose colour, and possessed all the properties of the cobaltic acid.

This acid when mixed with alcohol afforded an abundant precipitate, and the precipitate when collected presented all the phenomena exhibited by the concrete cobaltic acid.

It appears therefore certain from the results I have here detailed, that the so called cobaltic acid has no existence; that the simple combination of arsenic acid with oxide of cobalt has led Citizen Brugnatelli into an error, because this combination with excess of acid, is the substance obtained from zaffre by the methods described in this memoir.

in excess of acid.
A direct combination of oxide of cobalt and arsenic acid, had the colour, the precipitability, and all the properties of the supposed cobaltic acid.

Inference.

XIV.

*On the Plumb Line and Spirit Level, by EZEKIEL WALKER.
Communicated by the Author.*

Lynn Regis, March 18, 1802.

WHEN it is considered that the accuracy of astronomical observations depends greatly upon the exact manner of adjusting the instruments, any improvement in the method of making those adjustments may not be thought unworthy of a place in a philosophical Journal.

Importance of the adjustment of instruments.

The plumb-line and spirit-level are much used by astronomers, but from the great improvements made in the construction of the latter, the plumb-line seems to lose that preference which it had some years ago.

Plumb-line and spirit-level.

Mr. Ludlam says *, "I believe the spirit-level cannot ascertain the position of the axis" (of an instrument) "nearer than to a minute if so near." And when I first began to use the spirit-level for determining the position of the axis of a transit telescope, I found it nearly in the same imperfect state as mentioned by Mr. Ludlam: I was therefore very desirous of having some better method of making this adjustment.

Common spirit level said not to exceed the precision of one minute.

After having thought much upon the subject, I fixed on having a plumb-line suspended from the object end of the

Plumb-line to a transit instrument.

* *Astronomical Observations*, page 42.

telescope,

telescope, to pass over a point near the eye end. And in this manner a plumb-line was applied to it by Mr. E. Troughton, with his usual accuracy.

Described by Dr. Usher. This way of levelling the axis of a transit telescope has since been described by Dr. Usher in the Transactions of the Royal Irish Academy.

Laid aside. The Dr. speaks unfavourably of it, but many of his objections are ill founded, and it is probable that he never tried it. This method of applying the plumb-line gave me much satisfaction at that time, but some years afterwards it was totally laid aside for a ground spirit-level. The spirit-level which I now use is ground to a radius of 430 feet, with two indexes made of ivory, each about one-third of a minute in length, graduated to every two seconds. These are made to slide upon the level, that they may be brought over the ends of the bubble at pleasure; and no other adjustment is necessary.

Improved method of using it. My method of using the spirit-level is not only more convenient than any other that I have met with, but the axis of an instrument may also be examined by it without altering the perpendicular screw, which is a matter of some consequence; because an instrument newly adjusted is subject to vary for some time after, by the changes which take place in the temperature of the air; consequently the adjusting screws should never be touched, except to make some necessary alteration.

Observe one end of the bubble; reverse the level, and observe again. The half difference of station is the error; to which bring the instrument. My method is as follows. Having hung on the level, slide an index over one end of the bubble, and after it has remained at rest about a minute read off from the index the number of seconds *a* pointed out by the end of the bubble, then take off the level, invert the ends, and hang it on again, and if the same end of the bubble come to the same division (*a*) as before, the axis is level. But if the end of the bubble rest at any other division (*b*) on the index, then half the difference will be the error; and by means of the perpendicular screw, bring the end of the bubble half way between *a* and *b*, and the axis will be level without further trouble.

If the position be also determined by the other end of the bubble, the difference is seldom half a second. The position of the axis may be also determined at the same time, by the other end of the bubble, and if the level be a good one, the results given by the two ends will seldom differ so much as half a second.

In

In consequence of an accident which happened to my spirit-level, I was under the necessity of having recourse once more to my plumb-line.

But Mr. Nicholson's paper on page 134, Vol. I. of his Journal, (quarto) made me look upon it with a very suspicious eye. My having been assured that my wire was of the best quality, did not remove those suspicions which that paper had raised in my mind of its being crooked. At last a method of trying this by experiment occurred to me, which was by reversing the sides of the wire, thus: after having made the wire to bisect the point near the eye end of the telescope, I turned that side of the wire which faced the south, to face the north, which was done very easily, by reversing that piece of brass to which the wire is fixed at the top, and at the same time turning the plumb line half round the same way. To my great surprize I found that my wire did not form an accurate right line by seven seconds, though it was loaded with a proper weight.

I tried another piece of wire from the same bobbin, which deviated six seconds from a right line; I tried several other pieces before I found one that stood the test of this examination.

But after the error of a plumb-line has been thus determined, it is as good as if it were perfectly straight.

Another inconvenience that attended my plumb-line was this,—though the wire hung as near the point as possible without touching, yet the microscope could not be adjusted to view them both distinctly at the same time. An accident, however, put it in my power to remove this difficulty. Levelling the axis of the transit, one day, without any light in the room, except that which came through the opening in the roof behind me, I observed the shadow of the wire upon the telescope. By placing the microscope a little on one side of the wire, I saw the shadow very black and perfectly well defined. The shadow and the point being on the same plane, both objects were distinctly seen with the same adjustment of the microscope.

The length of this plumb-line from the notch in which the wire rests at the top to the point is about $41\frac{1}{2}$ inches. The wire subtends an angle of 24 seconds at the point, and the point itself 52 seconds, consequently four seconds of the point are seen on each side of the shadow when the wire bisects the point.

Plumb line returned.

Suspicion of flexure in the wire.

How verified.

The error from flexure was seven seconds.

Remedy. But qu. if the error be invariable? W. N.

Difficulty of observing the wire and the stroke at the same time.

Removed by using the shadow of the wire.

Estimate error of adjustment by this plumb line.

PLUMB LINE AND SP. LEVEL.

Though these eight seconds measure only .00159 parts
inch, yet an experienced observer will estimate with
able exactness when the parts of the point seen on
side of the shadow are equal. Still it is not improbable,
a second or two may sometimes escape his notice on fo
a scale.

he spirit-level has the advantage of so large a scale, that a
nd may be divided by the eye into many equal parts, and
so easy in its application, that a few minutes only are suf-
ficient for determining the position of the axis of an astrono-
mical instrument, to less than a single second, as appears by
the following observations.

Position of the Axis shewn by the Spirit-Level.

By the end x.	By the end y.	"
1.75 -	- 1.7	$x + y \div 2 = 1.72$
1.65 -	- 1.85	$x + y \div 2 = 1.75$
2.1 -	- 2.2	$x + y \div 2 = 2.15$
1.7 -	- 1.77	$x + y \div 2 = 1.73$
1.6 -	- 1.6	$x + y \div 2 = 1.6$

East end of the axis dips - - - - 1.79

taken These ten observations are independent of each other, they
- repeated. were all taken at the same time, and none were rejected to
 make a show of precision, instead of shewing the truth.
rels of I have had other spirit-levels ground by the same ingenious
on artist *, which were as good as this, though one of them was
accurate. filled with spirits of wine.

* Mr. Edward Troughton, Fleet-Street, London. He in gene-
ral uses ether.

XV.

*Description of a Machine for raising Ore from Mines. By Mr.
T. ARKWRIGHT *, of Kendal, Westmoreland.*

A, Plate XV. is an endless chain formed of thin plates of iron, through the ends of which plates iron bolts are passed, which keep the sides of the chain at a certain distance asunder, and on which the buckets to contain the ore are suspended. **BCDE**, the buckets suspended on the iron bolts, **GHI**, three cylinders, round which the chain and buckets revolve. The two cylinders **GH** are placed above the shaft; the cylinder **I** within the mine. Their rims are so much higher than the body of the cylinders, as to admit the buckets to lie within the rims.

As the endless chain and buckets are moved forwards by a power applied to the axis of the cylinder **G**, the bolts of the chain fall into notches made at regular distances in the rims of that cylinder, which preserve the chain from slipping.

As each empty bucket passes under the axis of the bottom cylinder **I**, it loads itself with ore instantaneously from a large box **K**, constantly filling by the workmen below, which box rests on two moveable pins **L**, at that end furthest from the wheel, and on an iron catch **M** at the other. The bucket thus filled ascends to the top of the cylinder **G**: and, in its passage betwixt the cylinders **G** and **H**, discharges its contents into a channel or receiver placed betwixt them, from whence they slide into a cart or receptacle placed underneath the inclined trough **N**. The empty bucket passes over the cylinder **H**, descends on the opposite side under the cylinder **I**, and loads itself again at **K**, as before mentioned; the buckets regularly loading and discharging themselves, whilst the cylinder **G** is kept in motion.

O is a ratchet-wheel on the cylinder, to prevent a retrograde motion in the chain.

* From "Transactions of the Society of Arts" for 1791. p. 277. The Society gave Mr. Arkwright a premium of twenty-five guineas for this invention.

ON PRIME NUMBERS.

shews, upon a larger scale, the manner in which K above mentioned loads the buckets. P is an iron projecting from the endless chain, which, pressing upon catch R, underneath the box K, occasions that part of box next the chain to sink down, and discharge into the bucket beneath it a quantity of ore sufficient to fill it. As the loaded bucket rises, it lifts the box K to its former place, till operation is repeated by the next tooth upon the chain.

XVI.

Curious Properties of prime Numbers, taken as the Divisors of Unity. By a Correspondent.

To Mr. NICHOLSON.

SIR,

IN the fourth volume of your Journal, page 403, (4to.) you inserted a curious property respecting the quotients resulting from the division of an unit by all prime numbers, not less than the number 7. I shall now lay before you two other properties equally curious, arising from similar divisions, and shall be much gratified, if in the hands of some of your learned readers or correspondents, this communication should, as I trust it will, prove really beneficial.

I am, SIR,

Yours, &c.

H. G.

March 22, 1802.

PRIME NUMBERS.

Law of the quotients of unity \div by prime numbers.

1. The first half of the quotient figures arising from the division of an unit by any prime number, not less than the number 7, will be the complements of the other half, to the number 9.

EXAMPLE

ON PRIME NUMBERS.

EXAMPLE I.

$$1 \div 7 = .142857$$

or

$$1 \div 7 = \left\{ \begin{array}{ccc} 1 & 4 & 2 \\ 8 & 5 & 7 \end{array} \right. \begin{array}{l} . \quad . \quad 1\text{ft.} \\ . \quad . \quad 2\text{d.} \end{array} \left. \right\} \text{half.}$$

$$\text{Quotient figures added} = \underline{\underline{999}}$$

EXAMPLE II.

$$1 \div 29 = .0344827586206896551724137931$$

or

$$1 \div 29 = \left\{ \begin{array}{ccc} .03448275862068 & . & 1\text{ft.} \\ 96551724137931 & . & 2\text{d.} \end{array} \right\} \text{half.}$$

$$\text{Quotient figures added} \quad \underline{\underline{99999999999999}}$$

Remark.—From hence it appears, that if half such like quotients be found by division, the other moiety may be obtained by a simple addition only.

2. The first half of the dividend figures (or successive remainders) arising from the division of an unit by any prime number, not less than the number 7, will be the complements of the other half to the prime divisor. Low of the remainders of unity ÷ by prime numbers.

EXAMPLE III.

The prime divisor being 17.

$$\text{Then } 1 \div 17 = .0588235294117647$$

or

$$1 \div 17 = \left\{ \begin{array}{cccccccc} 1 & 10 & 15 & 14 & 4 & 6 & 9 & 5 \\ 0 & 5 & 8 & 8 & 2 & 3 & 5 & 2 \\ 16 & 7 & 2 & 3 & 13 & 11 & 3 & 12 \\ 9 & 4 & 1 & 1 & 7 & 6 & 4 & 7 \end{array} \right. \begin{array}{l} 1\text{ft. half of div.} \\ 1\text{ft. half of quot.} \\ 2\text{d. half of div.} \end{array}$$

$$\text{Divs. added} = \underline{\underline{17 \ 17 \ 17 \ 17 \ 17 \ 17 \ 17 \ 17}}$$

$$\text{Quots. added} = \underline{\underline{9 \ 9 \ 9 \ 9 \ 9 \ 9 \ 9 \ 9}}$$

EXAMPLE

ON PRIME NUMBERS.

EXAMPLE IV.

prime divisor being 23.

$$1 \div 23 = .0434782608695652173913.$$

or

$$\div 23 = \left\{ \begin{array}{l} 1 \ 10 \ 8 \ 11 \ 18 \ 19 \ 6 \ 14 \ 2 \ 20 \ 16 \text{ divs.} \\ .0 \ 4 \ 3 \ 4 \ 7 \ 8 \ 2 \ 6 \ 0 \ 8 \ 6 \text{ quots.} \\ 22 \ 13 \ 15 \ 12 \ 5 \ 4 \ 17 \ 9 \ 21 \ 3 \ 7 \text{ divs.} \\ 9 \ 5 \ 6 \ 5 \ 2 \ 1 \ 7 \ 3 \ 9 \ 1 \ 3 \text{ quots.} \end{array} \right.$$

rs. added	23	23	23	23	23	23	23	23	23	23	23
	9	9	9	9	9	9	9	9	9	9	9

Remark.—From hence it appears, if half the dividend be used (and they are easily obtained by the continual addition of a few of the first quotient figures) the remainder regularly exceeds them as complements of the former.

Observation.—The result of the divisions of an unit by the numbers 11, 31, 37, 41, &c. appears at first sight to depart from the general law above laid down, but in fact it is subservient to it, as will appear from the following

EXAMPLE.

The prime divisor being 41.

$$\left\{ \begin{array}{l} 1 \ 10 \ 18 \ 16 \ 37 \text{ dividends.} \\ .0 \ 2 \ 4 \ 3 \ 9 \text{ quotients.} \\ 40 \ 31 \ 23 \ 25 \ 4 \text{ dividends.} \\ .9 \ 7 \ 5 \ 6 \ 0 \text{ quotients.} \\ 2 \ 20 \ 36 \ 32 \ 33 \text{ dividends.} \\ .0 \ 4 \ 8 \ 7 \ 8 \text{ quotients.} \\ 39 \ 21 \ 5 \ 9 \ 8 \text{ dividends.} \\ .9 \ 5 \ 1 \ 2 \ 1 \text{ quotients.} \\ 3 \ 30 \ 13 \ 7 \ 29 \text{ dividends.} \\ .0 \ 7 \ 3 \ 1 \ 7 \text{ quotients.} \\ 38 \ 11 \ 28 \ 34 \ 12 \text{ dividends.} \\ .9 \ 2 \ 6 \ 8 \ 2 \text{ quotients.} \\ 6 \ 19 \ 26 \ 14 \ 17 \text{ dividends.} \\ .1 \ 4 \ 6 \ 3 \ 4 \text{ quotients.} \\ 35 \ 22 \ 15 \ 27 \ 24 \text{ dividends.} \\ .8 \ 5 \ 3 \ 6 \ 5 \text{ quotients.} \end{array} \right.$$

A table of the quotients and dividends arising from all prime numbers under 1000 is nearly completed, and will be at the service of the public whenever it shall appear sufficiently beneficial.

SCIENTIFIC

SCIENTIFIC NEWS.

*Extract of a Letter from Mr. W. WALKER, Lecturer on the
Eidouranion, on the new Planet Ceres.*

To Mr. NICHOLSON.

S I R,

42, Conduit-street, Hanover-square,
London, 25th March, 1802.

THE interest that I find excited, both in public and in private, by the discovery of a new planet, induces me to offer you the latest observations by which it can be ascertained, that the necessary publication of your most valuable Journal will admit.

Since the 15th of this instant March, I have regularly observed it; and by the following memorandums it will be readily found by any of your readers, who are solicitous, and provided with a night telescope, or a common spy-glass, for it is not visible to my unassisted eye.

*Observations on
the position of
the neighbouring
stars, by means
of which the
planet may be
easily found.*

If at 9. 10. 11. 12. 1. 2. or 3. in the night, a line is drawn from Theta Lionis, through Beta or the Lion's Tail: at the same distance, and to the left, but a little above this termination, a small configuration of stars will be perceived of the following description: Two stars of considerable brightness will be found to form one side of an equilateral triangle, and a very small star the other point of this figure. Between these larger ones and the point will be seen two minuter stars, which, with the star at the point, form also an equilateral triangle. The Ceres is passing through this cluster. On the 15th instant the planet was to the east of the smallest star at the point of the triangle. I have regularly observed it on the 16th, 20th, 21st, 22d, and this evening the 25th, when it is arrived between the two westernmost or largest stars of the above configuration.

By continuing this line it may easily be discovered for a few nights to come. The planet is as bright as most of the stars in its neighbourhood. But although I have observed it with magnifying powers from forty to above one thousand times, I must hesitate in declaring it to have a defined diam-

ter

ter or disc, different from the neighbouring stars; whilst the Georgium Sidus in its neighbourhood leaves no doubt of a decisive magnitude.

I am, SIR, Your's, &c.

W. WALKER.

The following is the right ascension and declination of the planet (Ceres), by Mr. Zach of Gotha, for the commencement of the next month *.

	Right Ascension in Time.	Right Ascension in Degrees.	Declination North.
April 3	12 0' 12"	180° 3'	18° 6'
6	11 57 54	179 29	18 9½
9	— 55 45	178 56	18 10
12	— 53 46	178 26	18 9
15	— 51 37	177 59	18 5
18	— 50 20	177 35	17 59

Extract of a Letter from M. ZACH, Director of the Observatory of Gotha, to C. MECHAIN, of the Institute, Administrator of the Observatory at Paris.

Disc of the planet Ceres.

Suspicion of two satellites.
Dense atmosphere.

M. Schroeter, at Lilienthal, observes with his great telescope the disc of the new planet Ceres to be about two seconds. He also suspects the existence of two satellites. The planet is enveloped in a very thick atmosphere, for it appears to be surrounded with much nebulosity. I am very curious to know what Dr. Herschel will tell us concerning it. In the mean time, I thought it proper to write this in haste, &c. &c.

Elements.

Elements of the new planet, corrected by M. Gauss, from the latest observations:—

Diurnal heliocentric tropical motion 770" 7376
Tropical revolution 1681 days 12 hours 9 sec.
Decade Philos. No. 15. An. x.

* A small map of the stars above described was given in Mr. Walker's letter; but time did not admit of engraving it. W. N.

Spider's

Spider's Webs to form the cross Wires in the Eye Piece of Astronomical and other Instruments.

The smallest silver wire I have heard of was rather thinner Smallest wires than one thousandth part of an inch; but every specimen I have seen was considerably thicker. This has not been supposed of much consequence in the construction of plumb lines; but it is very desirable that the wires in the focus of microscopes should be as fine as possible. In a description of instruments in the Cabinet of the Grand Duke of Tuscany, arranged by the Abbé Fontana, and written by Ami Angand in one of the *Journals de Physique* above twenty years ago, to which I must refer the reader to the index of that useful work, not having a whole set at hand; mention is made of spider's webs Spider's webs used in microscopes. in the focus of microscopes for measuring; and long since, when upon observing that the single thread of the silk worm is very slender, I mentioned its use to my old friend J. H. de Silk worms threads. Magellan, F. R. S. he informed me, that organized substances had been tried in astronomical telescopes, and rejected, because the solar image was found to burn them.

I suppose this must have been taken for granted by himself Troughton's application of spider's webs to astronomical telescopes. or somebody else without trial; for Mr. Edward Troughton, whose talents are indefatigably directed to every thing that can insure the precision and accuracy of astronomical instruments, has for some time applied the webs of young spiders to the purpose above-mentioned. He takes hold of a short length of the web by means of a forked stick, and then carefully applies it to the marks on the frame or ring intended to hold it, where it adheres by the previous application of a little varnish. These lines have a beautiful aspect for their delicacy, precision, and evenness.

That the solar focus should not injure them is a fact of some Curious fact that the burning lens does not destroy small wire. remark; which I should be disposed to ascribe to their transparency, if I had not been assured by an eminent philosopher, that fine metallic wires were not affected by Parker's great lens, which instantaneously fused and burned thicker wires and masses. What may be the theory of this? Will the ascending current of air cool a small wire more speedily than a larger, Conjecture. of which the internal parts bear a greater proportion to the surface? or does the extreme light ignite and perforate the small

small wire, so as to escape with rapidity on the opposite surface, and keep down the maximum of temperature? &c.
Opera defiderantur, non verba. W. N.

Thick Iron burned in Oxygen.

Combustion of
iron in oxygen.

Mr. Accum informs me, that a thick piece of iron or steel, such as a file, may be burned in oxygen gas, if it be made very sharp pointed, and a small piece of wood be stuck upon its extremity, which is to be set on fire previous to immersion into the gas.

Extreme accuracy of tuning Musical Instruments.

Very exact pro-
cesses for tuning
musical instru-
ments.

About a year ago — Huddleston, Esq. shewed me an excellent monochord, with improvements for ascertaining the precise lengths, and securing the equable tension of the string, which I should be glad if I had an opportunity of communicating. I was much gratified by his method of tuning unisons to a degree of precision which the direct power of the ear could assuredly never command. 1. He tunes his string (*a*) (to the tone given by a fork, or otherwise) so as to have no beat or undulation, but to be as nearly unison as the sense can distinguish; 2. he tunes another string (*b*) a very small quantity sharper (or flatter) so as to afford an evident beat; 3. he sets a small pendulum, or bullet and thread, to vibrate with the beat; 4. he tries his fork with the last string (*b*) and observes the beat by the pendulum, unaltered; 5. if the beat be slower than before, it follows that the first string (*a*) is still too flat, because the fork is nearer unison with the sharp string (*b*); — if quicker, then (*a*) is too sharp. I heard four forks struck in quick succession, which had been tuned by this process; and though there was only the fundamental, with its third, fifth, and octave, the effect was delightful.

Strange influ-
ence of organ
pipes upon
each other.

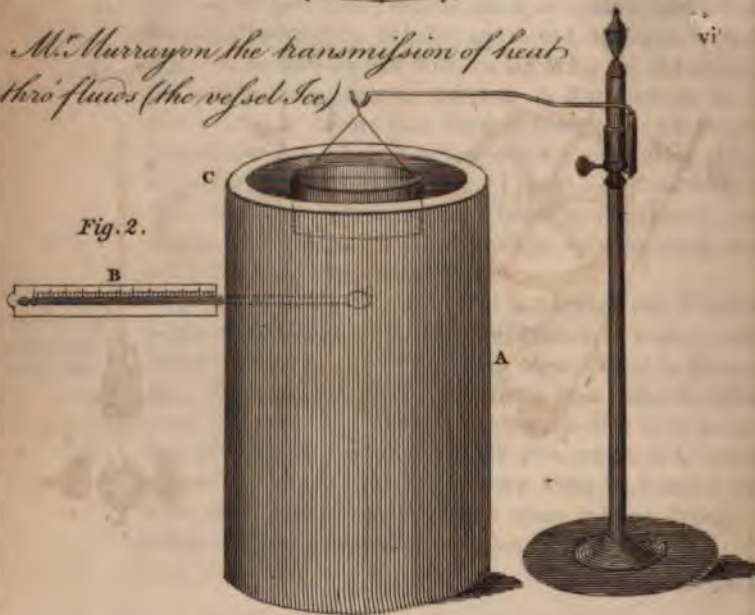
The same gentleman mentioned a singular, and I believe unobserved fact, relative to the combination of musical undulations in the air. Two contiguous organ pipes, both excellent, stopped diapason C and C sharp, in a valuable instrument he possesses, govern each other, so that if C be first sounded and held, the C sharp will give unison to C whenever its key is struck: but if C sharp alone be sounded and held, the C pipe will give C sharp when its key is struck. I will not venture to speculate upon this.

INDEX.

Rev. M^r Pearson's account of the Planet Ceres



M^r Murray on the transmission of heat thro' fluids (the vessel &c)



M. J. Delafont's Watch Escapements

Fig. 5.



Fig. 3.



Fig. 1.



Fig. 2.

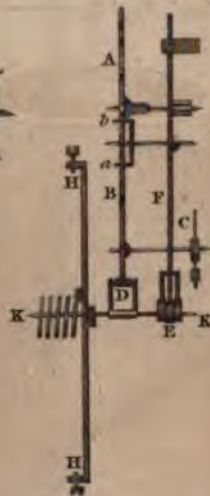


Fig. 4.





*M.^r T. Arkwright's Machine for
raising Ore from Mines.*

Fig. 1.



Fig. 2.



*Machine for determining the positions
of objects in drawing from Nature.*





INDEX.

A.

ABERNETHY, Mr. 26
Accum, Mr. Fred. his entertaining chemical experiments, with new facts and observations, 295.—On combustion of thick iron in oxygen, 320
Acetite of barites in crystals, 298
Achard, 204
Acid, fluoric, how to obtain it pure, 299.—benzoic, in vanellœ, 300.—Of cobalt, Darracq's proof that it is arseniate of cobalt, 238, 304, 309.—Gallic, Fiedler's method of rendering it pure, 236.—Muric, 235.—Sebacic, Cit. Thenard on, 34.—Zoonic, stated to be merely the acetic, 238
Adamantine spar found in America, 151
Agricultural Society of the department of the Seine, 157
Agriculture, memoirs of, 79, 160
Air, its influence in vegetation, 157
Alban, 119
Albumen used for clarifying, 256
Algaroth powder, 24
Amber, its excellence as a vehicle for oil pigments, 261.—Floating, in liquids is not made to circulate by currents, 82
Amoreti, opuscoli scelti da, 78
Antimonialis, pulvis, 22
Apparatus, hydraulic of Mr. Wm. Clofe, 27, 145.—For bleaching, 121, 128.—For shewing the transmission of heat through fluids, 167
Argand, 319
Arkwright, Mr. T. his machine for raising ores out of mines, 313
Arsenates of copper and iron, 258
Assignats, French, 118
Astronomy, Gregory's, 75
Athenas, 119
VOL. I.—MAY, 1802.

Atmosphere, its component parts little variable, 41, 43
Atlas of the south of Europe, by Chaulaire, 79
Aubert, Mr. 193

B.

Babington, Dr. 26
Ballie, 71, 104
Banks, Sir Joseph, 130
Barbers, Persian and Chinese, 50
Barites, acetite of, in crystals, 298.—Nobilis, 151
Barometer, why it varies so little near the equator, &c. 278.—Why farmers disregard it, *ib.*—Rules for it, 279
Battery, electric, charged by galvanism, by Volta, 140.—Large, charged with galvanism by Von Marum, 155
Baumé, 114
Beddoes, Dr. 43.—On the weather, &c. 98
Beet sugar, treatise on, 75
Bennet, Rev. A. 143.—Early experiments made by him on electricity excited by contacts of metals, 184
Benzoic acid, in vanellœ, 300
Bergmann, 34
Berthollet, 24, 43, 109, 118, 119, 235, 303
Biot, Cit. on the motions of camphor on water, 51
Blair, Mr. his popular lectures, 240
Bleaching of paper by Løysel, 148
Blue oxide of iron, native, 153.—Prussian, Des Mortiers on, 75
Blindness, recovery from, with some curious incidents, 60
Bode, 194, 196
Boerhaave, 204
Bones,

INDEX.

Bones, Proust on the quantity of nutriment they afford, 100.—Of fish, afford one sixth part more phosphoric acid than those of quadrupeds, 299
 Bonhomme, M. 25
 Book-keeping, Kelly's, 156
 Books, catalogue of, 160
 Borda, his circle of reflection, 6
 Boffut, on arches, 69
 Boncher, 159
 Bournon, Count de, 258
 Bourlier, 303
 Bouvard, 70
 Boyle, Robert, 265
 Bradley, Dr. 59
 Bread and butter, 150
 Brugnatelli, on the motions of odorant bodies on water, 51.—His supposed cobaltic acid, 304
 Bruckmann, Dr. 151
 Buffet, Stephen, 97
 Burkhardt, Cit. 194

C.

Cadet de Vaux, 157, 160, 212
 Camphor, Cit. Prevost on its remarkable motions on water, 51.—Motionless on mercury, 301.—Does not move on water at a low temperature, ib.
 Caloric, Socquet on, 76
 Capper, Col. on winds, 75.—On the causes that influence the weather, 275
 Carangeau, his graphometer, 112
 Carny, 119
 Carp, its air bladder when recent contains common air, and not azote, 299
 Carradori, 51, 52.—On the influence of oxygen on germination, 204
 Carriages of every description may be conveyed upon rail roads, 222
 Cataracts, case of recovery from blindness by, 57
 Cavallo, 144.—On the electricity produced by contact of metals, 184
 Cavendish, 43
 Cels, 159
 Ceres, the new planet, 72, 193.—A popular account of the methods of ascer-

taining its elements, &c. by the Rev. Wm. Pearson, 284.—Its place, 318.—Suspicion of two satellites, ib.—Very dense atmosphere—elements of Gauss, ib.
 Chamberlayne, Mr. 59
 Chancey, 160
 Clanlaire's atlas of the south of Europe, 79
 Chaptal, 118, 160
 Charcoal, used by Davy for galvanism without other solids, 144
 Chaffiron, 159
 Chaumontel, 159
 Chenevix, Rich. Esq. on James's powder, and a similar preparation in the humid way, 22.—On muriatic acid, 235, 258
 Chelfelden, comparison of his famous case of blindness, and another more recent case, 61
 Chemistry, elements of, 75
 Chlorophane, 151
 Chromate of iron discovered in France, 238
 Cinnabars, experiments on, 113.—In the humid way, 299
 Circles of reflection, of Mayer, Borda, and Mendoza, 4
 Cline, Mr. 105
 Cloquet, Cit. 281
 Close, Mr. Wm. his hydraulic apparatus acting by the syphon, 27.—the same analyzed, 145
 Clouds, planetary and luminous of the sun, 14, &c.
 Cobaltic acid is merely the arsenical, 238
 Colours how depraved by siccativ oils, 261
 Columbium, a new metal, 52
 Comet, 69
 Conductors of heat, 241
 Cooper, Mr. Astley, on deafness, and the perforation of the membrana tympani, 102
 Copal, its excellence as a vehicle for oil pigments, 262
 Copper, arseniate of, 258

Copying

INDEX.

Copying process, 147
 Corindon found near Philadelphia, 151
 Corrugations, solar, 18
 Coffigny, 160
 Coulomb, on magnets, 71
 Cream used to clarify spirits, 257
 Crell, 34.—His process for obtaining seb-
 acic acid described and examined, 37,
 151
 Creusé Latouche, 160
 Crichton, Dr. 26
 Cruickshank, Mr. Dr. Priestley's reply to
 his observations, 181
 Crystals, instrument for measuring their
 angles, 132.—Le Blanc's method of
 obtaining them very beautiful and large,
 191
 Currents in hot liquids whether they exist,
 by Dr. Thomson, 81

D.

Davy, Mr. H. his new eudiometer with
 sulphate of iron, 41.—On the absorp-
 tion of nitrous gas by the sulphate and
 muriate of iron, 107.—Lectures on
 chemistry, 135.—His pile for galva-
 nism with one metal only, 143.—With
 two metals, and the current reversed by
 changing the fluid, *ib.*—On a galvanic
 combination with charcoal and fluids,
 144.—New and curious inflammation
 of combustibles by, 296, 297
 Darcet on a pigment with cheese, 212
 Dark glasses, their place supplied by li-
 quids, 21, 224
 Darracq, Cit. on the supposed cobaltic
 acid, 238.—His examination of the
 supposed cobaltic acid, and experiments
 shewing it to be arsenic acid and cobalt,
 304, 309
 Deafness, Mr. Astley Cooper on, 102
 Decandolle, 205
 De Lafons, Mr. his free escapement for
 time pieces, 251
 Delambre, Cit. account of the transactions
 of the Institute of France, 69
 Delametherie, 135
 Delineation of objects, advantages of an
 early habit of, 282

Delong, 159
 Denorgelles, *ib.*
 De Sauffure, 143
 Descroizilles, 123
 Dijon, process of for obtaining sebatic acid
 described and examined, 37
 Discovery of new planets, how accom-
 plished, 285
 Dolomieu, on the art of making gun-
 flints, 88
 Doubler, Electrical, 185
 Drains, cause of their offensive smell, 280
 Drying oils, Mr. Shelstake on, 259
 Duc-la-Chapelle, 70

E.

Earth eaten by certain savage nations,
 152
 Edgeworth, Maria, 281
 Edgeworth, R. L. Esq. on the practica-
 bility of a general system of rail-roads,
 221.—His cheap, simple, and portable
 instrument, for taking objects in per-
 spective, 281
 Electricity from mere contact of metals,
 135, 184
 Electrometer of Volta, 136
 Elements of Chemistry by Murray, 75,
 239
 Emerald found in France, 258
 Engine for raising water by pressure, by
 Mr. Trevithack, 161
 Englefield, Sir Henry, on the planet Ceres,
 197
 Entertaining experiments, 295
 Escapement for watches, a new and free
 by Mr. De Lafons, 251
 Eudiometer, new, by Davy, 41
 Euler, Mr. 1.
 Eustachian tube, remedy for its closure,
 105

F.

Fat, acid of, Cit. Thenard on, 34
 Fiedler on the purification of gallic acid,
 236
 Filtering stone, a bad contrivance, 255
 Filtration, process of, 229, 253

M. J. Delafont's Watch Escapement

Fig. 5.



Fig. 3.



Fig. 1.



Fig. 2.

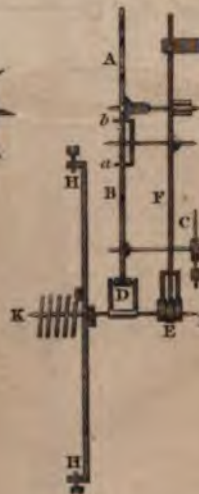


Fig. 4.



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3



*M.^r T. Arkwright's Machine for
raising Ore from Mines.*

Fig. 1.



Fig. 2.



A
JOURNAL
OF
NATURAL PHILOSOPHY,
CHEMISTRY,
AND
THE ARTS.

VOL. II.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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1802.

PREFACE.

I MEET my Readers and Correspondents at the completion of this Second Volume, with the satisfaction of directing their notice to the still greater increase of communication, and the means of rendering this work most eminently interesting and useful. It is not necessary that I should recapitulate the outlines of the plan and rules of conduct by which I have endeavoured to deserve the encouragement of the Public; but I think it truly justifiable to exhibit the marks of that approval at the same time that I express the sensibility with which I accept them, and the spirit they must afford to my labours.

Without entering into any estimate of the quantity of original matter in this work, which from the high value of its correspondence is now become the authentic repository of the researches of our philosophers; I will only notice, that the number of original writers in this Volume is more than double that of the respectable list prefixed to the first Volume, at the same time that the value and importance of the Memoirs from foreign and domestic publications have continued to increase.

The authors of original Papers are Mr. F. Accum; A. B. C.; John Bostock, M. D. J. W. Boswell; Count de Bournon; H. Campbell, M. D.; R. Chenevix, Esq.; Mr. John Clennel; C. P.; Mr. Wm. Close; W. Cruickshank, Prof. at Woolwich; John Cuthbertson; D. H.; J. Fletcher, Esq. G. H.; John Gough, Esq.; Mr. Olinthus Gregory; J. C. Hornblower; Rob. Jameson; J.; Rev. W. Pearson; N. N. Joseph Priestley, L. L. D. F. R. S. &c.; H. Sarjeant, Esq.; Mr. Tho. Sheldrake; Dr. J. H. Schroeter; Tho. Thomson, M. D.; Mr. Trevithick; Troughton; Ez. Walker; Rev. James Wilson, D. D. James Woodhouse, M. D.; Mr. Arthur Woolf; Thomas Young, M. D. Prof. P. R. I.; Baron von Zach; and W. N.—Of foreign works, Carcel; Carreau; Coulomb; Descroizilles; Guillot; Guyton; Hassel Lachenaie; Lalande; Piçet; Proust; Valentine; Vauquelin.—And of English Memoirs abridged or extracted, Mr. Banks; Wm. Bullock; Sir H. Englefield, Bart.; Mr. Gilpin; J. Gough, Esq.; Ch. Hatchett, Esq.; Wm. Herschel, L. L. D. Ed. Howard, Esq.; Hulme, M. D.; E. Jones, Esq. H. Sarjeant, Esq.

The novelty and excellence of the communications with which this Journal has been honoured, have been productive

PREFACE.

tive of an effect which I have seriously meditated to remedy. In the limited extent of every work of this nature, when the new and interesting Memoirs demand a larger portion of its capacity, a smaller must of course be devoted to less striking, though doubtless very important business of selection. Foreign and domestic matter must be more fastidiously sorted out; articles must be abridged instead of being given at full length; and some must be rejected altogether that would have been highly acceptable, if the original productions could have allowed room.

Two remedies present themselves. The first is to print a supplementary number to each volume; and the other to give a greater number of pages without adding to the price. I shall be happy to adopt the latter as soon as the increased sale shall have rendered it practicable, without diminishing the ordinary remuneration the Work affords; and in case the former should prove necessary or advisable, I am confident my Readers will see the advantages and approve the proceeding. In the mean time, the private recommendation of men of merit to their friends, is the best means of accelerating that circulation which will eventually benefit its patrons, by the greater quantity of them that could in that case be afforded.

I conclude this Preface as usual, by mentioning the subjects of the sixteen Plates which illustrate the present Volume. 1. Improvements in Hydraulic Engines, by Mr. Boswell. 2. Guyton's Improvements of the Swedish Stove. 3. Mr. Sarjeant's cheap Engine for raising Water. 4. Strong framed Levers for Steam Engines, by Mr. Hornblower. 5. Dr. Young's Diagrams to illustrate the Theory of Light. 6. Mr. Gregory's Figure for Mr. Pearson's Analogy. 7. Mr. Gough's Illustration of the Doctrine of Sound. 8. Mechanical Lamp of Carcel and Carreau. 9. Mr. Woolf's Apparatus for heating Water by waste Steam. 10. Mr. Terry's improved Mill. 11. Mr. Bullock's Lock. 12. Count Bourmon's Figures of Anhydrous Sulphate. 13. Mr. Trevithick's Application of a temporary Forcer. 14. Lachenaie's Apparatus for claying Sugars. 15. Mr. Jameson's Illustration of the Formation of Granite. 16. Mr. Banks's Instruments for determining the Pressures and Velocities of effluent Air or Gas. 17. The Spirit Hydrometer and Scales of Atkins. And, 18. Compound Condensers of Electricity, by Mr. Read and Mr. Cuthbertson.

Soho Square, London, September 1, 1802.

TABLE OF CONTENTS

TO THIS SECOND VOLUME.

MAY 1802.

F NGRAVINGS of the following Objects: 1. Mr. Boswell's Improvements in the Hydraulic Engines at Schemnitz, of Mr. Goodwyn, and that which acts by Pressure; 2 and 3. Two Copper Plates to exhibit the economical Swedish Stove, as constructed by Guyton; 4. Mr. Serjeant's very cheap Engine for raising Water for Domestic Purposes.	
I. Improvements in the Hydraulic Engine of Schemnitz, and in that of Mr. Goodwyn; with comparative Remarks on the most useful Applications of each, and some Facts relative to the Invention of the pressure Engine. In a Letter from Mr. John Whitley Boswell.	Page 1
II. Remarks on the present State of Paper-making in England and France. By H. Campbell, M. D. Communicated by the Author.	3
III. Remarks on Combustion, by Thomas Thomson, M. D. Lecturer on Chemistry in Edinburgh.	10
IV. Some Account of a new Planetary Body, discovered by Dr. Olbers, on the 28th of March, 1802.	20
V. On Bradley's Method of observing Transits, and another Method by which the Thickness of the Wire is rendered of no Importance. In a Letter from Mr. Ezekiel Walker.	22
VI. Description of a Stove on the Principles of the Swedish Fire-place, with Heat-openings, by Citizen Guyton.	24
VII. A Continuation of the Experiments and Observations on the Light which is spontaneously emitted from various Bodies; with some Experiments and Observations on Solar Light, when imbibed by Canton's Phosphorus. By Nathaniel Hulme, M. D. F. R. S. and A. S.	31
VIII. Caution against the great Danger of keeping Phosphorus in Bottles without particular Caution. By Descroizilles the Elder.	41
IX. Observations in Answer to Dr. Priestley's Memoir in Defence of the Doctrine of Phlogiston. In a Letter from Mr. William Cruickshanks.	42
X. On the new Planet Ceres.	48
XI. Description of a very cheap Engine for raising Water. In a Letter from Mr. H. Serjeant, of Whitehaven, to Mr. Taylor, Secretary to the Society for the Encouragement of Arts.	60
XII. Concerning the Identity of Tellurium and Antimony, the galvanic Effects of Magnetism, and other Philosophical Subjects. By a Correspondent.	62
Scientific News. Notice respecting the Discovery and Situation of chromated Iron in France.	64
Books of Science. A Treatise on Astronomy, in which the Elements of that Science are deduced in a natural Order, from the Appearances of the Heavens to an Observer on the Earth; demonstrated on Mathematical Principles, and explained by an Application to the various Phenomena.	ib.

VOL. II.—1802.

a

JUNE

JUNE 1802.

Engravings of the following Objects: 1. Strong framed Levers for Steam Engines, by Mr. J. C. Hornblower, and others; 2. Diagrams by Dr. Young, to explain the Nature and Properties of Light; 3, 4. Figure by Mr. Gregory to demonstrate Mr. Pearson's Analogy for deducing the greatest Equation from the Eccentricity: and another by Mr. Gough, for shewing how we perceive the direction of Sound. And, 5. A new Lamp by Messrs. Carcel and Carreau, in which the Oil is raised mechanically.

- I. On the Rev. Mr. Pearson's Analogy for deducing the greatest Equation from the Eccentricity. In a Letter from Mr. Olinthus Gregory. Page 65
- II. On the Construction of the Beams of Steam Engines. By Mr. J. C. Hornblower. From the Author. 68
- III. On the Theory of Chemistry. In a Letter from the Rev. J. Priestley, L. L. D. F. R. S. &c. 69
- IV. Experiments upon the tanning Principle, and Reflections upon the Art of Tanning. By Cit. Merat Guillot, Apothecary at Auxerre. 70
- V. On the Destruction of the Grub of the Cock-chaffer. By Edward Jones, Esq. of Wepre-Hall, in Flintshire. 73
- VI. Methods of diminishing the Irregularities of Time-Pieces, arising from differences in the Arc of Vibration of the Pendulum. By Mr. Ezekiel Walker. 76
- VII. On the Theory of Light and Colours. By Thomas Young, M. D. F. R. S. Professor of Nat. Philos. in the Royal Institution. 78
- VIII. Remarks on Combustion. By T. Thomson, M. D. Lecturer on Chemistry in Edinburgh. 92
- IX. A Continuation of the Experiments and Observations on the Light which is spontaneously emitted from various Bodies; with some Experiments and Observations on solar Light, when imbibed by Canton's Phosphorus. By Nathaniel Hulme, M. D. F. R. S. and A. S. (Concluded from page 40.) 100
- X. Description of a Lamp upon Argand's Principle, with Improvements, in which the Oil is maintained at the same Level by the constant Action of a Pump. By Citizens Carcel and Carreau 108
- XI. Note upon a peculiar vegetable Principle contained in Coffee. By Richard Chenevix, Esq. F. R. S. M. R. I. A. From the Author. 114
- XII. Account of some Experiments performed upon a Scale of considerable Magnitude, and principally by the Agency of Frost, to produce Sulphate of Soda, Carbonate of Magnesia, and Muriate of Ammonia, from Sulphate of Magnesia, Carbonate of Ammonia, and Muriate of Soda. By H. Campbell, M. D. From the Author. 117
- XIII. Note of Citizen Vauquelin respecting the Boracite, called Magnesio-calcareous Borate by the French Chemists. 120
- XIV. Facts and Observations tending to explain the curious Phenomenon of Ventriloquism. By Mr. John Gough. 122
- XV. An Analysis of a Mineral Substance from North America, containing a Metal hitherto unknown. By Charles Hatchett, Esq. 129
- XVI. Remarks on the Mammoth. By Louis Valentine, Physician in Chief of the Army and Hospitals of America, of several National and Foreign Societies, resident in Nancy. 138
- Scientific News. Prizes of the National Institute of France—Astronomical Prize—Communications to the Royal Society respecting the Planet Ceres—Leonardo da Vinci—Experiments to prove that all Bodies, whatever may be their Nature, are obedient to the Action of Magnetism, and that this Action is sufficiently powerful to admit of being decided. 141 to 143

JULY

CONTENTS.

iii

JULY 1802.

Engravings of the following Objects: 1. A new Apparatus for heating Water to nearly the boiling Point by Means of Waste Steam, by Mr. Arthur Woolf; 2. An improved Mill, by Mr. G. Terry; 3. Mr. Bullock's Drawback Lock; 4. Crystals of Anhydrous Sulphate, by the Count de Bournon; 5. Application of a temporary Forcer, by Mr. Trevithick; 6. Lachenaie's Apparatus for claying Sugars.

I. Composition of Writing Ink, possessing the permanent Colour, and other essential Properties, of the Ink used for Printing. In a Letter from Mr. William Cloze.	Page 145
II. Of the Effects produced by the Vegetation of Plants in Atmospheric Air.	150
III. On the Theory of Light and Colours. By Thomas Young, M. D. F. R. S. Professor of Natural Philosophy in the Royal Institution.	162
IV. An Analysis of a Mineral Substance from North America, containing a Metal hitherto unknown. By Charles Hatchett, Esq.	176
V. On the Effect of Sound upon the Barometer. By Sir Henry C. Englefield, Bart. F. R. S. (From the Journals of the Royal Institution, No. 9.)	181
VI. On the Expansion of carbonated Hydrogen by Electricity. From a Correspondent.	184
VII. A new Process for claying Sugars, proposed by Cit. Hassel Lachenaie, Chief Apothecary of the Military Hospitals of Guadaloupe, to the Agents of the Consuls of the French Republic in the Windward Islands.	187
VIII. Description of the Crystalline Forms of the Anhydrous Sulphate of Lime, with some Observations on this Substance. By M. Le Comte de Bournon, Member of the Royal and Linnæan Societies of London. Translated from the Original; communicated by the Author.	190
IX. Analysis of Natural and Artificial Anhydrous Sulphate of Lime. By Rich. Chenevix, Esq. F. R. S. M. R. I. A. Communicated by the Author.	196
X. Abridgment of a Memoir of Mr. Proust, on Tanin and its Species.	198
XI. Description of an Apparatus for heating Water by waste Steam. Invented by Mr. Arthur Woolf.	203
XII. Description of an improved Drawback Lock for House Doors. By Mr. Wm. Bullock. From the Transactions of the Society of Arts, who adjudged a Reward of Fifteen Guineas to the Inventor.	204
XIII. Description of an improved Mill for grinding hard Substances. By Mr. Garnett Terry. From the Transactions of the Society of Arts, who adjudged the Silver Medal to the Inventor.	206
XIV. Remarks on Dr. Thomson's Theory of Combustion. By C. P. (Received June 15, 1802.)	ib.
XV. On certain Points of Nomenclature. By a Correspondent.	212
XVI. Duplicate Copy of a Letter from Baron de Zach to the Right Honourable Sir Joseph Banks, Bart. P. R. S. &c. transmitted to Mr. Edw. Troughton, and communicated by the Rev. J. Pearson; on the new Planet Ceres and Pallas, with the Elements of the Orbit of the former.	213
XVII. Method of applying a temporary Forcer to a Pump, so as to produce a constant Stream. By Mr. Richard Trevithick. From the Author.	216
XVIII. Experiments and Observations on certain stony and metalline Substances, which at different Times are said to have fallen on the Earth; also on various Kinds of native Iron. By Edward Howard, Esq. F. R. S. From the Philosophical Transactions, 1802.	ib.
Scientific News. Dimensions and Nature of the new Planets Ceres and Pallas. By Dr. Herschel—Extract of a Letter from the Rev. James Wilton, D. D. Minister of Falkirk.	221 to 222
Account of Books of Science. Memoirs of the Literary and Philosophical Society of Manchester.	222

AUGUST

AUGUST 1802.

- Engravings of the following Objects: 1. Figures to illustrate Mr. Jameſon's Observations on the Formation of Granite; 2. Mr. Banks's Instruments for determining the Preſſures and Velocities of effluent Air or Gas; 3. The Spirit Hydrometer and Scales of Atkins; 4. Compound Condensers of Electricity, by Mr. Read and Mr. Cuthbertſon.
- I. On Granite. By Mr. Robert Jameſon. Communicated by the Author. Page 225
 - II. Observations on the Conversion of Iron into Steel. In a Letter from Joſeph Priſtley, L. L. D. F. R. S. &c. 233
 - III. An Account of the Art of making Glue. In a Letter from Mr. John Clennel. 235
 - IV. On the Preparation of Indelible Ink. In a Letter from Mr. Thomas Shel-drake. 237
 - V. Observations on the Cauſes why a large Quantity of common Salt pre-vents Putrefaction, and a ſmall Quantity haultens it. By D. H. 249
 - VI. Account of the Methods by which Soda is at preſent prepared for the Engliſh Market; with other Observations. By Mr. Fred. Accum. From the Author. 241
 - VII. Compariſon of the French definitive Metre with an Engliſh Standard, brought from London by M. A. Picſet, one of the Editors of the Bibliothèque Britannique. 244
 - VIII. On the Figure of Sulphate of Barytes, and the Formation of Man-dreporce. In a Letter from Mr. H. Sarjeant. 253
 - IX. Experiments and Observations on certain ſtony and metalline Subſtances, which at different Times are ſaid to have fallen on the Earth; alſo on va-rious Kinds of Native Iron. By Edward Howard, Eſq. F. R. S. From the Philoſophical Tranſactions, 1802. 254
 - X. An Answer to Mr. Gough's Eſſay on the Theory of Compound Sounds. By Thomas Young, M. D. F. R. S. 264
 - XI. Experiments on the Velocity of Air iſſuing out of a Veſſel in different Cir-cumſtances; with the Deſcription of an Inſtrument to meaſure the Force of the Blaſt in Bellows, &c. By Mr. Banks, Lecturer in Natural Philoſo-phy. 269
 - XII. On the Variation of Rate in a Time Piece, as indicated by the Changes in the Arc of Vibration. In a Letter from Mr. Ezekiel Walker. 273
 - XIII. Deſcription of Atkins's Hydrometer for aſcertaining the ſpecific Gra-vities of ſpirituous Liquors. By J. Fletcher, Eſq. Communicated by the Author. 276
 - XIV. An Examination of Sig. Volta's Experiments which he calls funda-mental, and upon which his Theory of Galvaniſm reſts; with a Deſcription of a very ſenſible Electrical Condenſer, and an Explanation of the Action of the Electric Fluid in the Galvanic Inſtrument. By John Cuthbertſon, Phi-loſophical Inſtrument Maker, No. 54. Poland Street, London. Communi-cated by the Author. 281
 - XV. Observations on the Phosphoreſcence of the Tremolite, and of the cal-careous Phosphate of ſlow Solution, known by the Name of Dolomite. By M. le Comte de Bournon, Fellow of the Royal and Linnæan Societies. Tranſlated from the Original; communicated by the Author. 290
 - XVI. Outline of the Hiſtory of Galvaniſm; with a Theory of the Action of the Galvanic Apparatus. By John Boſtock, M. D. From the Author. 296

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MAY, 1802.

ARTICLE I.

Improvements in the Hydraulic Engine of SCHEMNITZ, and that of Mr. GOODWYN; with comparative Remarks on the most useful Applications of each, and some Facts relative to the Invention of the pressure Engine. In a Letter from Mr. JOHN WHITLEY BOSWELL.

To Mr. NICHOLSON.

S I R,

London, March 14. 1802.

HAVING, with much satisfaction, found that the method of making the Schemnitz hydraulic engine work itself, which I gave you for your excellent Journal in 1800, (IV. 117.) has been since found of considerable utility in * other works of a similar nature, I am induced to send for your approbation a draft of the † application of the same principle to Mr. Good-

Method of
working engines
by the bucket
and syphon.

* Vide Mr. Close's papers in the same Vol.

† The paper signed L in the quarto Journal, page 343, though it professes to shew how Mr. Goodwyn's engine may work itself, has only hinted at this method, but has not shewn how it may be effected.

wyn's engine, and another of a method of causing the Schemnitz to raise water *above* the level of the prime reservoir, together with a comparative view of the advantages of both engines and their powers.

Concise explanation of the Schemnitz engine.

After a perusal of my former Paper on the Schemnitz engine, (IV. 144.) a mere inspection of the figure given here (Plate I.) will be sufficient to shew the manner in which this now proposed will operate. The moving power is the pressure of the column of water from the reservoir R, (Fig. 1.) to D in the lower air chamber A, which forces the air contained in it into the chamber B, which air so compressed in B will impel the water contained in it upwards through the pipe to a height, and in a quantity proportionate to the relative height of the column of water contained in the pipe R D, compared with that contained in S B, or supposing the length of R D given, the greater the length of the pipe S B is, (so as not to exceed R D,) the less will be the quantity of water delivered at S, and *vice versa*.

Mr. Goodwyn's engine; constructed for heavy work, and made to operate without attendance.

In the draft of Mr. Goodwyn's engine, Fig. 2. I have endeavoured to exhibit it as it should be if executed on a large scale, and made all the pipes detached from each other, because though the plan of making one pipe pass through another and through the reservoirs, made use of in Mr. Goodwyn's model, is very convenient and neat in an apparatus that may be placed on a table, yet it would be found to produce an unnecessary trouble, complication, and difficulty of repair in a large engine. The method shewn in this draft of causing the engine to work without attendance, is the same as for the Schemnitz, and causes the cocks G and H to open at intervals, (which may be regulated at pleasure by the hand cock I, letting the water flow more or less quick into the syphon vessel E,) while at the same time it closes the cock at D, and *vice versa*. Self-moving valves are placed at the delivering pipes of the chambers C and B, and also at the air vent of A, because wherever they can be used they are preferable to cocks, or valves used by external power; some doubt may arise, whether there should not be a passage for the air let at intervals into A, as well as one for it to escape: but as great quantities of air are contained in water, which the mode of working of this engine will particularly tend to separate from it, I think it would be needless and that the self-moving valve

valve opening outwards at the air vent of A will be sufficient. The pipe at K is to conduct water from the bucket F to that of the cock D. There are two ranges of reservoirs represented, to shew the method of raising water by this engine about thirty feet high : more would be useless, and even a second would in very few instances be found necessary, except when the fall of water from R to D was very short, in which case it would be better to use some other engine for raising water to the required height.

In comparing those two engines, it will be found that their powers and capabilities are nearly similar.

Comparison of
the powers of
these two en-
gines ; numeri-
cally stated.

1. In both the greater the height of the original fall of water, denoted by the pipe R D, and the greater the quantity of water which it can supply in a given time, the greater quantity can be raised by these engines in a given time.

2. Both engines can be constructed so as to raise water above the original level, and from below to the surface, or from a pit.

3. By a successive number of reservoirs both engines can be brought to raise water to any height ; but as they will raise a smaller quantity as the height is increased, the quantity wanted in a given time, and the expence of construction, will limit the extent of their elevation.

4. In both engines the distance from one reservoir to another, must always be less than that of the original fall R D. The circumstances in which those engines differ arise from the difference of their manner of action.

5. The Schemnitz engine operates by causing a fall of water to *compress* air, which re-acting on other water forces it to rise in a pipe to a certain height. Mr. Goodwyn's engine acts by causing a fall of water to *rarefy* a certain quantity of air, into whose space the pressure of the atmosphere forces, when permitted, a quantity of water.

6. Hence in the Schemnitz engine, the pressure acting from *within outwards*, tends to *burst* the vessels used in the structure, and to *open* and *extend* any fissures which may chance to be in them.

7. In Mr. Goodwyn's engine the pressure acting from *without inwards*, closes all the parts of which it is composed more together, tends to make its pipes and vessels more staunch, and in any fissure makes its sides operate like valves to shut it up.

8. The Schemnitz engine will always raise water to a height nearly equal to that of the original fall, from one reservoir to another, supposing the original fall of any height whatsoever, as 100 feet.

Mr. Goodwyn's engine will not raise water from one reservoir to another so high as thirty feet in any case whatsoever, as there cannot be a complete vacuum formed by it in the air chamber, but only an approximation to one.

Mr. Goodwyn's engine will be preferable, and least costly in small elevations.

From this comparison it will follow, that wherever the original fall of water is less than thirty-two feet, Mr. Goodwyn's engine will be much preferable to the Schemnitz; as, from the 7th article of the comparison, it may be made of the *cheapest materials*, of strong *wooden casts* and *wooden pipes*; whereas the Schemnitz engine, from the 6th article, must be made of the strongest, and of course most costly materials, of cast iron at least, and that of considerable thickness.

The Schemnitz engine for greater depths.

But wherever the original fall exceeds the height of thirty feet much, and it is required to raise the water to nearly the same height, then the Schemnitz engine appears to be preferable; as, in all probability, the fewer number of parts which it will, in this case, require in its construction, will more than compensate for its costly materials.

Piston engine best for very great depths.

When it is required to raise water to a height, much greater than that of the original fall, above the first level, or from a greater depth; either from the original fall being short, or the required height being great, an engine in which the pressure of the water is made to act by a piston in an apparatus similar to that of the steam engine, (one of which is described in your Journal for March) will be preferable to either of the above.

Comparison of the three engines.

The comparison of those engines may be brought to this one point: wherever Mr. Goodwyn's engine can be used, with a single continued pipe for elevating the water, and without a succession of reservoirs, it seems to be the cheapest.

Where Mr. Goodwyn's engine cannot be used without violating this condition, but the Schemnitz can, it promises to be the next in point of cheapness, from its simplicity, absence of friction, and small number of working parts.

But when neither Mr. Goodwyn's engine nor the Schemnitz can be used without a number of reservoirs, then the piston pressure engine probably ought to be preferred; but this will much depend on the number of reservoirs, for perhaps one or

two in addition to the Schemnitz might cost less, than boring the cylinder of the piston engine perfect, and its additional machinery: for merely raising water the powers of each are nearly equal, depending all on the height of the original fall of water.

The great advantage of the piston pressure engine is, not as a cheap engine for raising water, but as that in which a fall of water can be applied without any waste to work mills or machinery for any purpose; which is of very great consequence when the fall of water is of considerable height, and the stream, or supply, small.

Conceiving it of very great importance to have it determined in what situations each of the principal engines, worked by water pressure, is to be preferred, I have commenced this comparison, and if this shall be acceptable, will send another paper on a similar subject; that is, on the comparison of the common mill water wheel, with another mode of applying the water to turn mill work, which, I think, I can demonstrate to be much preferable. I hope what I have thus began will excite some others to the same enquiry; and that by this means the multiplicity of water pressure engines will be at last arranged, and their comparative utility ascertained, so that in every different case of fall or supply of water, an engineer may know at once which he should use.

I beg leave to add here some remarks on the piston pressure engine in your Journal for March: Mr. Trevithack will, I hope, excuse my taking from him the honour of his being the first inventor of this mode of applying a fall of water, to give it back to Messrs. Denifard and Deuille of France, when I confess that for a long time I was in the same error with him, and thought it had first occurred to me, and proposed it with that idea to Mr. Carnac in Nov. 1796, to draw the water from his copper mine, (by which he would have saved the daily labour of twenty men, as he had a fall of water very proper for this engine—a fact which I have Mr. Carnac's signature to prove;) but I since found out that in Belidor's Hydraulic Architecture, published at Paris 1739, in the fourth book and first chapter, there is a method described at large, with very well executed plates, by which a fall of water operating in a cylinder on a piston may work a pump to force water to a greater height; and what is remarkable, Belidor

The pressure engine works without waste of water.

Great importance of comparing the different methods of applying water as a force.— Wheel work, &c.

Mr. Trevithack's pressure engine was executed in 1731 by Denifard and Deuille—and imagined by Mr. Boswell in 1796.

Described in Belidor.

propofes the very fame method of working the valves by a tumbling weight, as that called the tumbling bob, in Mr. T's defcription. In Belidor's engine the piston cylinder and pump are both horizontal, which is the moft material difference, but the principle is entirely the fame as Mr. Trevithack's.

Mr. Belidor does not claim the honour of this invention, but only propofes the engine I allude to, as an improvement on one executed on the fame principle at Seve, between Versailles and Paris, in the year 1731, by Denifard and Deuille, for which they obtained a patent from the king of France for twenty-one years; of which engine there is alfo a defcription in the fame chapter of the above work.

Propofal of air chambers in the piston.

I beg leave to fuggelt, that it might be a confiderable improvement to this engine to have its action made elastic, by the addition of an air chamber, on the fame principle as that ufed in engines for extinguifhing conflagrations; fuch a one, feems to me, might be beft effected by making the piston hollow, and of a larger fize, to ferve for this purpofe, as the air fpring would then act both on the upper and lower prefure of the water; Figure 3 is a fketeh of this method, in which A represents the hollow piston.

I hope the length of this paper will be excufed by the circumftance of my not having received your Journal for March till I had written the moft of it, and having had of courfe, to add the remarks on the piston prefure engine to the reft.

I am, SIR, &c.

W. H. B.

II.

Remarks of the prefent State of Paper-making in England and France.* By H. CAMPBELL, M. D. Communicated by the Author, March 26, 1802.

Whether the additional duty on paper has principally injured book trade.

THE additional duty on Paper has not been the chief caufe of the diminution in the book and paper trades of this country.

After

* The importance and authenticity of the chemical, mechanical, and other facts ftated in this Memoir, and of the object itfelf in general

After establishing this fact, it will be equally evident that the paper and book trades will not be *effectually* relieved by giving up a part, or the whole of the last additional duty.

The former administration of this country by imposing an additional duty, did not teach the French the new art of paper making; nor did it supply them with abundance of raw material; or shew them the art of equalizing the different qualities of rags by Chaptal's mode of bleaching;—neither was any new light thrown by the additional duty upon the mode of printing and type casting, as now practised in France.

France has succeeded by improvements in the arts of paper making and printing.

Before the commencement of the late war, paper-making in this country, strictly speaking, was a mechanical art. The superiority of English paper arose from the superior linen worn by English people. Their rags were superior to the rags collected on the continent. The decency of the English populace, compared with the populace on the continent, could not be better shewn than by an exhibition of English and foreign rags. Rags called *London fines*, the refuse of Irish linen, &c. could scarce be equalled in any other country. The original linen of these rags had been highly bleached. They were consequently calculated to make the finest and whitest paper. Rags at an English paper mill previous to the war, were *sorted* according to their colour and fineness. Pains were taken to cut off the seams and offal parts; and these parts were destined to make inferior paper. This statement alone is almost sufficient to give a clear notion of what I am about to establish.

The British fine paper formerly excelled from the fineness of our linen rags;

and our careful sorting them,

I conceive that a representation of the *mechanical necessities*, as the duster, the knives placed in the engine roller, the plate, the vat, the moulds, and lastly, flannels and presses, although indispensable in a paper mill, are necessary to be noticed in this account; as an imitation of them in France, could not contribute to the present alarming change in our paper trade.

Our machinery,

The English staple, superior rags, English cleanliness in attempted to be cutting and assorting, and better engines and knives, transferred to France.

general to the progress of science and literature, leave no doubt respecting the propriety of inserting it in our Journal, though its most prominent feature relates to political regulation. On this last subject my conduct as a Journalist cannot be supposed to express any opinion as an individual.---N.

tuted

tuted our superiority. About the year 1789 and 90, *certain respectable* English paper makers endeavoured to take these advantages to France. France apparently required them; her coarse foul cotton rags were not made better by separation and assortment; her engines were deficient, and her mills exhibited no mark of prosperity, but every symptom of slovenly neglect.

French rags are coarse cotton.

French paper for copper plates.

One sort of paper, notwithstanding the want of colour and cleanliness, she excelled in, upon necessity. I mean paper for copper plate prints; she necessarily excelled because her staple cotton rag being more bibulous, received better impressions from the plate. This was the state of her paper trade and mills in the year 1789. Before that period, she did not consume all her own rags. We received a considerable part from the cellars of Dunkirk and Ostend, and from countries in her southern vicinity, Leghorn, &c.

New æra in paper making. The bleaching process by ox. mur. acid.

About this time a new æra in paper making commenced. Chemistry by her disciples, Scheele, Berthollet, and Chaptal, from a metallic oxide, solicited and directed the concentrated and pure part of the atmosphere, oxygen, to remove with expedition the colouring part of cloth, or rags made from vegetable substances, such as flax, cotton, hemp, &c. An attention to the best bleaching process ought certainly to form a material part of the considerations in this Paper; because this object, connected with a knowledge of the sorts of rags suitable for bleaching, and making *printing* paper, would give that comprehension of the evil from which might be drawn—not steps of temporary and unavailing expedience, but solid foundations of relief.

Coarse cotton rags are brought to an equality with fine linen rags by that process.

The business under consideration is more intimately connected with printing than with writing paper. The bleaching gas is much better adapted to coarse cotton rags than coarse linen or hempen rags; because the former is without ligneous particles, and the latter abounds with them, and these particles, called by paper makers *sheaves*, are made more conspicuous by bleaching. The staple of our opponents the French, consists in coarse cotton rags. I can, if required, point out places where depots should be established for affording an unlimited supply of similar *cheap* materials, and where each ship, by way of finishing her lading, will take on board a convenient number of bags.

A plentiful and cheap supply of cotton rags.

In

In the year 1793-4, I imported a parcel of cotton rags at 9l. per ton; bleaching them added 8 per cent. to the 9l. The bleached stuff was worth more than 40l. per ton: the beautiful paper produced now exists in a public work. If paper makers, or stationer-paper makers, had paid attention to this new and growing improvement, (or would pay attention) instead of soliciting a remission of duties, they might be able with their *capital* and mills ready formed, to counteract the French, and contribute still more to the state, than is paid at present. A relaxation of part of the duties, cannot be a radical relief. To forego an object of finance without remedying a complaint, can only be a compliance with the government, to participate in misfortune with the petitioners.

The necessity of abolishing improper combinations and power among journeymen paper makers, and the injury done to paper mills by London stationers *importing and regulating the price of rags*, and monopolizing mills, are evident directions to relieve the suffering part of the trade, and how to obtain revenue from the wealthy part, by way of granting licences, I shall be happy, if required, to communicate.

The arts, commerce, and navigation of this country, are justly considered by the French government, to be the finews of England. Chaptal, Berthollet, and other enlightened men are encouraged to affect us, and benefit their own country in these particulars: that they have not been unsuccessful is manifested by the state of the paper trade in France.

The maxim of trade finding its level, is too supine a maxim in the present state of discoveries. Factories are altering, and markets must vary. Goods formed by mere *mixture*, such as saline and other bodies, require neither capital, nor machinery. Many of these are in a state of great cultivation in France. In arts resting on capital, machinery, and aptitude of hand, we shall long remain unrivalled.

At present I shall check these observations, that my remarks on the paper trade may remain distinct and unmixed with any other matter.

H. CAMPBELL.

No. 11, Fleet-Street, London.

III.

Remarks on Combustion, by THOMAS THOMSON, M. D. Lecturer on Chemistry in Edinburgh.

Phenomenon of combustion is very striking to every class of men.

Eminent investigators of its theory.

Lavoisier's theory

leaves much to be still elucidated.

NO operation of nature has a better claim to our attention than COMBUSTION. The irresistible devastation which it sometimes occasions is calculated to strike the ignorant with terror; the extraordinary changes which it produces naturally attract the inquisitive eye of the philosopher, while its subserviency to almost every branch of domestic economy renders it a familiar and necessary agent in the hands of every individual. This familiar acquaintance with combustion seems, however, to have retarded the investigation of its nature; for it was not till the seventeenth century that philosophers made it a serious object of enquiry. The labours of Bacon, Boyle, Hooke, and Mayow are well known; and the success with which these labours were attended, must, if we recollect the difficulties to be overcome, give us a very high idea of the genius of these investigators of nature. But the philosophers of our own age, especially Lavoisier, have gone far beyond their predecessors; and have explained some of the most intricate and important phenomena of combustion.

Mr. Lavoisier's theory of *combustion*, improperly termed his theory of *chemistry*, is so generally known, that it is unnecessary to enter into any detail concerning it. Its merit is indisputable, and raises its author to the very first rank among philosophers. Many chemists seem to think that it explains the whole phenomena of combustion; but an attentive examination must convince every impartial observer, that the theory of Lavoisier, ingenious and satisfactory without doubt, as far as it goes, leaves yet several parts of that very complicated process as unaccountable as ever. He has corrected the errors of his predecessors, and made a very important new step; but many new steps are still wanting to render the theory complete. I hope, therefore, that the following remarks will not be considered as altogether improper; they will at least exhibit the subject in a new point of view, and may perhaps contribute to call the attention of chemists to certain phenomena

phenomena which have not hitherto been classified, nor examined with that precision to which they are entitled.

1. Though the French chemists have lately given the term *combustion* a new meaning, and made it stand for the general combination of a body with oxygen, I mean, for reasons which will appear hereafter, to employ it in the sense usually affixed to the term by the generality of mankind. Now when a body undergoes combustion, in the common sense of the word, two things always take place. 1. The body gradually wastes away, and often disappears altogether; it is then said to be *consumed* or *burnt*. 2. During the whole of this process it emits *heat* and *light*; the heat and light thus emitted are usually denominated *fire*, and the waste of the body is considered as the effect or consequence of its combustion. If either of these two phenomena be wanting, we do not say in common language that a body is undergoing combustion, or that it is burning. Every theory of combustion then must explain, 1. Why the burning body is wasted and altered. 2. Why during the progress of this alteration heat and light are emitted.

The French confine the term combustion to the act of oxidation.

Usual acceptance of the term preferred, *consumption* or *waste* with *heat* and *light*.

The theory must explain these effects.

2. If we take a view of the different bodies which occupy the attention of chemists, we shall find, that as far as combustion is concerned, they may be arranged under three classes; namely, 1. Combustibles. 2. Supporters of combustion. 3. Incombustibles.

Relative to combustion bodies are 1. Combustibles, or 2. supporters of combustion, or 3. incombustible.

1. The COMBUSTIBLES are those bodies, which are said in common language to *burn*. During combustion they appear to emit light and heat, and at the same time gradually waste away. When this change has reached its maximum, the process of combustion is at an end. The class of combustibles is very numerous; but all the bodies belonging to it may be subdivided into three sets; namely,

1. Combustibles or the bodies consumed.

1. Simple combustibles,
2. Compound combustibles,
3. Combustible oxides.

The *simple combustibles* are twenty-four or twenty-five in number, namely,

They are simple combustibles; or

1. Sulphur,
2. Phosphorus,
3. Carbon,
4. Hydrogen gas,
5. All the metals*.

* Except perhaps gold, silver, and mercury.

The

—compound
combustibles,

The *compound combustibles* consist of compounds formed by the simple combustibles uniting together two and two; and are of course much more numerous than the simple combustibles. They may be arranged under the five following heads:

1. Sulphurets,
2. Phosphurets,
3. Carburets,
4. Alloys,

5. Sulphurated, phosphorated, and carbonated hydrogen.

or combustible
oxides,

The *combustible oxides* are composed of one or more simple combustibles, combined with a dose of oxygen. Though the French chemists have given to these bodies the name of *oxides*, we shall see afterwards that they differ essentially from *metallic*

and these last are
either simple,
having a single
base, or com-
pound, having
more than one
base.

oxides and from water, which is considered at present as an oxide of hydrogen. The combustible oxides may be arranged under two heads: 1. Those which contain only a single base combined with oxygen, and which therefore may be termed *simple combustible oxides*. 2. Those which contain more than one base combined with oxygen, and which therefore may be termed *compound combustible oxides*.

Simple comb.
oxides.

The simple combustible oxides are only four in number; namely,

1. Oxide of sulphur,
2. Oxide of phosphorus,
3. Charcoal,
4. Carbonic oxide gas.

Unless sulphur, phosphorus, and hydrogen gas, bodies at present considered as simple, belong to this class. All the simple combustible oxides are by combustion converted into acids.

Compound
comb. oxides.

The compound combustible oxides include by far the greater number of combustible bodies; for almost all the animal and vegetable substances belonging to them. The double base is usually carbon and hydrogen: alcohol, ether, resins, gums, &c. are instances of compound combustible oxides*.

Phlogiston.

It was believed by Stahl and his disciples, that all combustible bodies contain one common principle, to which they owe their combustibility. But in consequence of the discoveries of Lavoisier this theory has been laid aside.

II. *Supporters of
combustion*

II. The *supporters of combustion* are a set of bodies which are not of themselves, strictly speaking, capable of undergoing combustion, but which are absolutely necessary for the process; for no combustible body can be made to burn unless

* To this class of bodies also must be referred all the vegetable and animal acids,

some one or other of the *supporters* be present. Whenever they are excluded the process stops. All the supporters known at present are the following six :

- | | |
|----------------------------|----------------------|
| 1. Oxygen gas, | 4. Nitrous gas *, |
| 2. Air, | 5. Nitric acid, |
| 3. Gaseous oxide of azote, | 6. Oximuriatic acid. |

There are indeed certain substances besides these, which possess nearly the same properties; these I shall enumerate afterwards under the title of *partial supporters*.

All the supporters contain one common principle, namely, *universally contain oxygen*. The first of them consists of oxygen uncombined with any base; but in the other five the oxygen is united to a base. It is very remarkable, that in four cases out of five, the base to which the oxygen in these compound supporters is united is *azote*. Is it not probable from analogy, that oximuriatic acid, the remaining compound supporter, contains azote likewise as a component part.

N. B.

III. The *incombustible bodies* are neither capable of under-III. *Incombustibles*. going combustion themselves, nor of supporting the combustion of those bodies that are; of course they are not immediately connected with combustion. At present we are acquainted with about 13 incombustible bodies, not reckoning the compounds which they are capable of forming with each other. These are,

- | | |
|------------------------|----------------|
| 1. Azotic gas, | 3. The earths. |
| 2. The fixed alkalies, | |

The first of these substances constitutes the base of almost all the compound supporters. Some of the alkalies and earths possess certain properties in common with combustibles, and are capable of exhibiting phenomena somewhat analogous to combustion; phenomena to be described afterwards under the title of *semi-combustion*.

3. From the preceding observations it is obvious, that in every case of combustion there must be present a *combustible* and a *supporter*. Now during combustion the combustible always unites with the oxygen of the supporter. It is this combination which occasions the apparent waste and alteration of the combustible. The new compound thus formed I shall call a *product of combustion*. Now every product of combustion is

Combustion requires a combustible and a supporter.

Product of combustion is either water or an acid, or a metallic oxide.

* Mr. Davy first proved that this gas is a supporter.

either,

either, 1. *water*, or 2. *an acid*, or 3. *a metallic oxide*. It is true indeed, that other bodies sometimes make their appearance during combustion, but these will be found upon examination not to be products, nor to have undergone combustion.

Thus one of the two characteristic marks which distinguish combustion, namely, the apparent waste and alteration of the combustible body, has been fully explained. For the explanation of it we are indebted to Lavoisier. It constitutes what is usually, but absurdly, termed the *new theory of chemistry*, and is the most important step which has been made towards a complete theory of combustion,

Facility of combustion is not proportioned to the attraction for oxygen.

But chiefly depends on the facility of destroying the cohesion.

Hence compound supporters are more readily burned.

But though the combination of the combustible with oxygen be a constant part of combustion, yet the facility with which combustibles burn is not proportional to their apparent affinity for oxygen. Phosphorus, for instance, burns more readily than charcoal; yet charcoal is capable of abstracting oxygen from phosphorus, and of course has a greater affinity for it. The combustible oxides take fire more readily than some of the simple combustibles; thus charcoal burns more easily than carbon or diamond; alcohol, ether, and oils, are exceedingly combustible, whereas all the metals require a very high temperature when the supporter is air. This greater combustibility of combustible oxides is probably owing to the weaker affinity by which their particles are united. For the cohesion of heterogeneous particles, when oxygen constitutes a part of them, is usually weaker than the cohesion of homogeneous particles. Hence they are more easily separated than homogeneous particles, and of course combine more readily with oxygen; those simple combustibles which melt easily, or which are in the state of elastic fluids, are also very combustible, because the cohesion between their particles is easily overcome.

It is owing to the same inferiority in the cohesion of heterogeneous particles, that some of the compound supporters occasion combustion in circumstances when the combustibles would not be acted on by simple supporters. Thus phosphorus burns in air at the common temperature; but it does not burn in oxygen gas, unless the temperature exceed 90°. In oximuriatic acid gas phosphorus burns rapidly at the common temperature of the air, and so do several of the metals; though

though they cannot be made to burn in air except at a very high temperature. Thus also oils burn rapidly when mixed with nitrous acid. Nitrous gas and the gaseous oxide of azote constitute exceptions to this rule.

4. None of the *products* of combustion are combustible according to the definition of combustion which I have given. This want of combustibility is not owing to their being saturated with oxygen; for several of them are capable of combining with an additional dose of it. But during this combination no caloric nor light is ever emitted; and the compound formed differs essentially from a *product* of combustion; for by this additional dose of oxygen the *product* is converted into a *supporter*.

Products of combustion are never combustible.

Oxygenation of a product converts it into a supporter.

Hence we see that combustion ought not to be confounded with the combination of a body with oxygen, as is done by the French chemists. Combustion indeed cannot take place without the combination of oxygen; but oxygen may combine without combustion. Thus when iron is burnt, it always combines with 0.27 of oxygen, and is converted into the *black oxide*, a product of combustion, and altogether incombustible; capable, however, of combining with an additional dose of oxygen, and of being converted into the *red oxide*. But during this last combination, how rapidly soever it takes place, no heat nor light is emitted. Now the *red oxide of iron* is not a product of combustion, but a supporter; as the following experiments demonstrate; Mix it with phosphorous, and put the mixture into the bottom of a long glass tube, shut at one end, and filled with azotic gas. Close the mouth of the tube, and apply heat to that part in which the mixture is. At a certain temperature a violent detonation takes place, which shatters the tube in pieces. It is needless to remark, that the tube must be sufficiently long to prevent the effects of expansion in the gas included.

Difference between combustion and oxygenation.

Detonation of phosphorus with oxide of iron.

When antimony is burnt, it always combines with 0.20 of oxygen, and is converted into the *white oxide*. Now this white oxide, which is a product of combustion, and of course incombustible, is capable of combining with an additional dose of oxygen, and of being converted into the *acidulous oxide* of antimony. In like manner, lead, when burnt, is converted into the *white oxide* of lead, a product; but this product combines with

Other products converted into supporters.

with additional doses of oxygen, and is converted into the *red* and *brown* oxides, both of which are supporters. When the *They act by part of their oxygen*; supporters, thus formed by the combination of oxygen with *products*, are made to support combustion, they do not lose all their oxygen, but only the additional dose which constituted them supporters. Of course they are again reduced to their original state of products of combustion. Hence it follows, that they owe their properties as supporters, not to the whole of the oxygen which they contain, but to the additional dose which constituted them supporters. We may therefore call them *partial supporters*, indicating by the term, that part only of their oxygen is capable of supporting combustion, and not the whole. It is very possible that both *azote* and *muratic acid* may be products of combustion; and in that case both the compound and partial supporters would agree with each other in every respect. In the present state of our knowledge, however, it is necessary to distinguish them.

and are *partial supporters*.

The bases of all known *partial supporters* are metallic.

All the partial supporters with which we are acquainted, contain a metallic basis; for metallic oxides are the only products at present known capable of combining with an additional dose of oxygen. It is a circumstance highly deserving of attention, that when metals are capable of combining with several doses of oxygen, the product or oxide formed by combustion is seldom or never that which contains a maximum of oxygen. The following oxides are products of combustion:

Enumeration.

- | | |
|------------------------------|-------------------------------|
| 1. Black oxide of iron. | 8. Oxide of copper *. |
| 2. White oxide of zinc. | 9. Oxide of cobalt *. |
| 3. White oxide of lead. | 10. Oxide of nickel *. |
| 4. Yellow oxide of tin. | 11. Oxide of bismuth *. |
| 5. White oxide of antimony. | 12. Purple oxide of gold ? |
| 6. White oxide of arsenic. | 13. Yellow oxide of silver ? |
| 7. White oxide of manganese. | 14. Black oxide of mercury †? |

* The particular oxide of these metals, which is the product of combustion, has not been ascertained; but they are all combustible in oximuriatic acid gas.

† I doubt much whether gold, silver, and mercury, be combustible at all. They do not burn in air, how high soever the temperature is; neither do they detonate with red hot nitre, nor exhibit any appearance of combustion in oximuriatic acid gas; though this last body oxidates them with great rapidity.

The

The following oxides, on the other hand, are partial supporters of combustion :

- | | |
|-----------------------------|-------------------------------------|
| 1. Red oxide of iron *. | 6. Red and brown oxides of lead **. |
| 2. Yellow oxide of gold †. | |
| 3. White oxide of silver ‡. | 7. Black oxide of manganese. |
| 4. Redoxide of mercury §. | 8. Acidulous oxide of antimony? |
| 5. Arsenic acid . | 9. White oxide of tin ? |

This list would doubtless be increased by an accurate examination of all the metallic oxides not included in either of these tables.

Thus it appears that several of the products of combustion are capable of combining with oxygen. The incombustibility of products, therefore, is not owing to their want of affinity for oxygen, but to some other cause.

5. No product of combustion is capable of supporting combustion. This is not occasioned by any want of affinity for combustible bodies ; for several of them are capable of combining with an additional dose of their basis. But by this combination they lose their properties as products, and are converted into combustibles. The process therefore differs essentially from combustion. Thus sulphuric acid, a product of combustion, by combining with an additional dose of sulphur or its oxide, is converted into *sulphureous acid*, a substance which, from several of its properties, I conclude to be combustible. Thus also phosphoric acid, a product of combustion, is capable of combining with phosphorated hydrogen, and of forming *phosphorous acid* a combustible body. When this last acid is heated in contact with a supporter, it undergoes combustion ; but it is only the additional dose of the combustible which burns, and the whole is converted into phosphoric acid. Hence we see that it is not the whole basis of these compounds which is combustible, but merely the additional dose. The compounds, therefore, formed by the union of a product and combustible,

Incombustibility of products is not owing to want of affinity for oxygen.

Nor can they support combustion, though they can combine with combustibles.

Instances. This combination affords partial combustibles, acting by part of their basis.

• Fulminates with phosphorus.

† Forms fulminating gold.

‡ Forms fulminating silver.

§ Forms fulminating mercury.

|| Occasions combustion when heated with several combustibles.

** Occasions the combustion of sulphur.

Since *products* can combine with oxygen, but never exhibit combustion unless they be *partial combustibles*, combustibles must contain a substance which they lose in burning. *Products* can give oxygen to *combustibles*, and convert them into *products*; but they do not, like *supporters*, cause combustion.

The oxygen of *supporters* differs from that of *products*. Combustibility is restored to *products* only by *combustibles*.

Doctrine of
Stahl.

may be termed *partial combustibles*; indicating by the name, that a part only of the base is capable of undergoing combustion. Now since the products of combustion are capable of combining with oxygen, but never exhibit the phenomena of combustion except when they are in the state of partial combustibles, combustible bodies must contain some principle which they lose during combustion, and to which they owe their combustibility; for after they have lost it, they unite to oxygen without exhibiting the phenomena of combustion.

Though the products of combustion are not capable of supporting combustion, they not unfrequently part with their oxygen just as supporters do, give it out to combustibles, and convert them into products; but during this process no heat nor light is ever evolved. Water, for instance, gives out its oxygen to iron, and converts it into *black oxide*, a product; and sulphuric acid gives out its oxygen to phosphorus, and converts it into phosphoric acid. Thus we see that the oxygen of products is capable of converting combustibles into products, just as the oxygen of supporters; but during the combination of the last only, are heat and light emitted. The oxygen of supporters then contains something which the oxygen of products wants.

6. Whenever the whole of the oxygen is abstracted from products, the combustibility of their base is restored as completely as before combustion; but no substance is capable of abstracting the whole of the oxygen, except a *combustible* or a *partial combustible*. Water, for instance, is a product of combustion, whose base is hydrogen. To restore the combustibility of the hydrogen, we have only to mix water with iron or zinc filings; the metal is oxidated, and the hydrogen gas is evolved as combustible as ever. But no substance, except a combustible, is capable of separating hydrogen gas from water by combining with its oxygen. In the same manner phosphorus absorbs the oxygen from sulphuric acid, and is converted into a product, while sulphur is separated in its usual state of combustibility. Thus we see that combustibles are capable of restoring the combustibility of the bases of products, but they themselves lose their combustibility by the process, and are converted into products. Combustibility, therefore, may be thrown at pleasure from one body to another. This fact was first set in a proper light by Stahl, and was the great step in the theory of combustion.

combustion for which we are indebted to that philosopher. Some mistakes into which he fell were afterwards corrected by Lavoisier*.

From these facts it is obvious, that the products of combustion may be formed without combustion; but in these cases a new combustible is always evolved. The process is merely an interchange of combustibility; for the combustible is converted into a product only by means of a product. Both the oxygen and the base of the product having undergone combustion, have lost something which is essential to combustion. The process is merely a double decomposition. The product yields its oxygen to the combustible, while at the same time the combustible gives out something to the base of the product; the combustibility of that base then is restored by the loss of its oxygen, and by the restoration of something which it receives from the other combustible thus converted into a product.

When products are thus formed without combustion, a new combustible is evolved.

There is indeed another method of forming the products of combustion without actual combustion in certain cases; but the phenomena are much more complicated. This method is to expose them to the action of some of the supporters dissolved in water; especially nitric acid. Thus most of the metallic oxides may be formed without combustion by the action of that acid on the metals. But in that case a new supporter is always evolved, namely, nitrous gas; ammonia, a new combustible, is usually also formed; and not unfrequently the product is converted into a partial supporter.

Complicated case of products formed by nitric acid, &c.

7. No supporter can be produced by combustion, or by any equivalent process. Now as all the supporters, except oxygen gas, consist of oxygen combined with a base, it follows as a consequence, that oxygen may combine with a base without losing that ingredient, whatever it is, which gives occasion to combustion. The act of combination of oxygen with a base, therefore, is by no means the same with combustion. If we

Oxygen may combine without losing the ingredient which occasions combustion.

* When sulphate of iron is dropt into a solution of muriate of gold, nitrate of silver, or nitrate of mercury, the gold, silver, and mercury are precipitated in the metallic state. This is an additional reason for suspecting that these three metals are not combustible. Every person, however, must have observed, that the metals in question have not the metallic lustre when first precipitated, and that they acquire it slowly when allowed to remain exposed to the light.

All supporters taken view of the different supporters, we shall find that all of them which can be obtained artificially, are procured either from other supporters, or by the agency of electricity.

Oxygen. I. OXYGEN GAS may be procured from nitric acid and oximuriatic acid, two supporters; and from several of the partial supporters, as the black oxide of manganese, the red oxides of lead and of mercury. The action of heat is always necessary; but the process is very different from combustion.

Air. II. AIR, as far as is known at present, cannot be formed artificially. The gas indeed which comes over during part of the usual distillation of nitre and sulphuric acid to obtain nitrous acid, resembles air very closely. But it is obtained from a supporter.

Oxide of azote. III. THE GASEOUS OXIDE of azote, or nitrous oxide of Davy, has hitherto been only procured from nitrous gas and nitric acid (nitrate of ammonia), both of which are supporters.

Nitrous gas. IV. NITROUS GAS can only be procured by the decomposition of nitric acid, a supporter.

Oximuriatic acid. V. OXIMURIATIC ACID can be formed by combining muriatic acid with the oxygen of nitric acid, a supporter; or with the oxygen of the black oxide of manganese, the red oxides of lead, iron, mercury, &c. all of which are partial supporters.

Nitric acid formed spontaneously. VI. NITRIC ACID is formed spontaneously upon the surface of the earth by processes with which we are unacquainted.

(To be continued.)

IV.

*Some Account of a new Planetary Body, discovered by
DR. OLBERS, on the 28th of March, 1802.*

Discovery of a
new planet.

A LETTER from Dr. J. H. Schroeter, of Lilienthal, to Mr. Best, London, was lately read at the Royal Society, in which he gives an account of this remarkable discovery of a second planetary body of small apparent magnitude, discovered by Dr. Olbers, of Bremen, on the 28th of last March, when it formed the fourth point of an equilateral triangle with the stars Nos. 20 and 19 of the constellation Virgo. During the comparisons he was induced to make, from his conviction that it was not there in January when he re-discovered the Ceres

near

near the same spot, he found its change of place to be very perceptible. The following observations were transmitted by him to Dr. Schroeter :

d.	h.	m.	s.	
1802.	Mar.	28	9 25 10	M. T. App. R. Af. $184^{\circ} 56' 49''$ Its place,
				App. Dec. 11 33 30 N.
		29	8 49 14	M. T. App. R. Af. $184^{\circ} 46' 36''$
				App. Dec. 11 52 59 N.

Dr. Olbers had not the means of observing any disc, but Dr. Schroeter, from his information, began to observe it on the 30th with his thirteen feet reflector, while his assistant, Mr. Harding, determined its place. With a power of 288 of this large instrument, it appeared round and less hazy than Ceres, with a diameter of 4.635 seconds, which is much larger than those of Ceres and the Georgium Sidus; the former of which, on the 28th March, measured 4.021 sec. and the latter, on the 20th, measured 3.973 seconds. Its light, though pale and white in comparison with that of Ceres, was nevertheless more intense upon the whole, as appeared by its projection on the disc-microfometer; but the Georgium Sidus was much brighter.

A minute star was seen near it on two several days, which Dr. Schroeter seems to suspect as a satellite.

The position of the Olberian planet (or comet), at March 30 d. 8 h. 20 m. 50 sec. mean time, was

App. R. A. $184^{\circ} 35' 52''$ and $12^{\circ} 15' 8''$ app. N. declin.

On the 1st of April it was again observed with the great reflector, and appeared to exhibit no disc, but was less in a brighter light with the power of 288, and could not be distinguished from a fixed star. When it sometimes appeared with a disc, its diameter was only $3''.244$. The Doctor is disposed to ascribe this to the heavy dew.

Letter from Mr. W. WALKER, Lecturer on the Eidouranion.

To Mr. NICHOLSON.

41, Conduit Street, Hanover Square, London,
April 26, 1802.

SIR,

THE account you did me the favour of inserting in your valuable Journal of the last month, respecting the situation of the planet Ceres, will be rendered additionally interesting at present, from the circumstance of a still more recently observed

Present situation, April 26.

Definable disc with power of 100. Pale red and less brilliant than Ceres. Supposed to be nearer the Earth.

moving star being situated very near to the situation there pointed out as the place of the Ceres on the 25th of March. The planet or comet discovered by Dr. Olbers, at Bremen, on the 28th ult. is at present in a very small degree to the S. E. of the situation of the Ceres on the 25th of March, and will readily be seen by a night-glass or telescope. It is invisible to my naked eye, but appears of a definable disc with a magnifying power of a 100 times. Its light is pale red and very faint, and through the night-glass is less brilliant than the Ceres, although no magnifying power that I can use will give the latter any apparent diameter. It seems probable that its distance is about as far again as the Earth is from the Sun, whilst the Ceres is near three times the distance, and Mars about once and a half as far off. As my object is merely to enable any person to find it, I do not trouble you with any more full account at this late period of the month.

I remain, SIR,

With much respect,

Your constant reader,

W. WALKER.

Present situation of Ceres.

The Ceres Ferdinandia will be found a little to the north-east of the star Beta in the Lion's Tail, being the easternmost point of a right angled triangle formed by Beta, a double star due north of it and itself.

V.

On Bradley's Method of observing Transits, and another Method by which the Thickness of the Wire is rendered of no Importance. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR,

Dr. Bradley's method of making transit observations described.

THE method of taking transit observations introduced by Dr. Bradley is still used by astronomers. This consists in noting the proportional distance of the star from the wire at the two beats of the clock, one immediately preceding, and the other immediately following its passage across the wire. If the wire be so thick as to cover the star, and the star happens to be behind it when the clock beats, the situation of the star's

star's centre cannot be exactly known, which makes the introduction of fine wires very desirable. The finest wires still proving too thick for very small stars, an astronomical friend of mine hinted to Mr. Troughton that he might probably receive assistance in this delicate branch of his business from some of his spiders. This hint was not lost,*—and hence Bradley's method of observing seems to be carried to the highest degree of perfection by the assistance of a harmless insect which is persecuted without mercy by every *fille de chambre* throughout his Majesty's dominions.

Spider's webs in the foci of telescopes.

There is, however, another method of observing which precludes the necessity of very fine wires. This consists in noting the time when the centre of the star comes to the side of the wire. But before this method is used it is necessary that one side of the middle wire should be brought into the meridian; suppose it be that side which appears to the west when the telescope is turned to the south, then the observations are to be taken on that side of all the wires.

Another method of observing one side of the wire.

It is a line drawn by the strength of imagination down the middle of the wire, parallel to the sides, which is used in Bradley's method, but in my method a *real line* is presented to the eye of the observer and which he sees very distinctly, although as fine as if it had been drawn through a geometrical definition.

Which is a real line.

This method of observing seems to be more simple than the other, in consequence of its being, in many cases only necessary to attend to one side of the wire: for example: Should the clock beat when half the star is covered by the wire, it is evident that the centre of the star passes the side of the wire at that time. And by making use of the apparent diameter of the star, and the thickness of the wire as two measures, the fractional part of a second may be estimated, by an experienced observer to a very great degree of precision.

Its advantages.

How far this small alteration in the method of using the transit telescope may be found convenient to others experience must determine; but for my own part, I am certain that I can observe, not only with more ease to myself, but with greater exactness by this method than by Bradley's.

I am, Sir,

Your very humble servant,

Lynn, April 19, 1802.

EZEKIEL WALKER.

* See a further account of this invention in the last vol. of this Journal, pa. 319.

VI.

*Description of a Stove on the Principles of the Swedish Fire-place, with Heat-openings, by CITIZEN GUYTON.**

Fire-places in France not generally constructed on good principles.

Often too deep.

Smoky chimneys how usually remedied.

General principles with regard to fires for domestic purposes.

THE true principles of constructing fire-places, so as to obtain the greatest heat with the least consumption of fuel, have been known for some time in France; but they have been much less generally adopted, than the necessity for economising fuel demands. We see many fire-places so deep as to consume double the quantity of fuel necessary, and yet heat the apartment but faintly, where half the expence might be spared by altering the fire-place according to count Rumford's plan.

If a chimney smoke, instead of reducing the tunnel to proper dimensions, so that descending currents cannot take place in it, scarcely any remedy is thought of but air-holes, which require the sacrifice of a certain quantity of fuel, to counter-balance the effect of the cold air continually entering.

The use of the Swedish stoves is probably yet rare, from their not having been constructed on just principles, or in the best proportions, at their first introduction. As I have had one made, which appears to many of my friends to produce an astonishing effect, in compliance with their request I shall give an exact description of it, premising however a few principles with regard to fires.

1. The heat produced is proportionate only to the air consumed by the fuel.

2. The quantity of heat produced by a given quantity of fuel is greatest when the combustion is most complete.

3. The combustion is most complete when the filiginous part of the fuel is retained longest in pipes in which it may undergo a second combustion.

4. Of the heat produced none is of use, but what is diffused through the space to be heated, and retained in this space.

5. The temperature in this space will be higher, in proportion as the current of air, which is to renew and keep up the combustion, is less disposed to absorb the heat of this space in passing through it.

* Abridged from the *Annales de Chimie*, vol. xli. p. 79. C.

Hence

Hence we deduce the following obvious consequences: Corollaries deduced.

1. The fire-place must be kept separate from all bodies that conduct heat rapidly.

2. As heat can be produced only by combustion, and combustion can be maintained only by a current of air, this current should be attracted into pipes, where it preserves the requisite velocity, without going away from the place to be heated; so that the heat it deposits in it gradually accumulates in the whole of the isolated stove, to be afterward given out slowly, according to the laws of its equilibrium.

3. When the wood is consumed to such a point as to afford no more smoke, it is of advantage to stop the outlets of these pipes, to keep in the heat, which would be carried into the chimney by the continued current of fresh air, which would necessarily be of a lower temperature.

4. We shall obtain a higher temperature, and preserve it longer, under similar circumstances, if we construct within the stove, or under the hearth and round the fire-place, pipes in which the air derived from without is warmed before it enters into the apartment to support the fire, or to replace what has been consumed. Apertures for heated air.

These pipes are what have been called heat openings, (*bouches de chaleur*;) because instead of considering their principal object, it is commonly supposed, that they are made to give a more rapid passage to the heat produced. This is not totally without foundation, since the temperature of the air issuing from them is increased by the heat it absorbs from the stove; and on this account some might be disposed to neglect them, as contrary to the most essential object, that of retaining the heat in it; but it is to be observed, that we can shut these outlets when we please; and that we may even cut off all communication with the external air by means of a simple slider; so that every advantage may be derived from them without any inconvenience. It must be added, that they are necessary in very close apartments, unless we would expose ourselves to currents of cold air. These reasons have induced me to employ the heat openings in the Swedish stove, to which they had not been applied. Necessary in very close apartments.

The Swedish stoves are constructed strictly according to the truest principles, and the pipes in which the smoke circulates are disposed in the best manner for effecting its complete combustion. Swedish stoves constructed on the best principles.

combustion.

So useful, that they are become general in Sweden ;

and are employed in different works.

Construction ;

and method of using them.

Description of the Swedish stove constructed by Guyton.
Its height,

breadth,

depth.

Height may vary.

Proportions of the circulatory pipes.

bustion. Their utility has been found so great, that they have become general in Sweden, where the winters are very severe, and where they have diminished the consumption of wood one third, so that there is no country where the inclemency of the weather is guarded against at less expence. They have likewise been employed advantageously, with the necessary variations of form, in dye-houses, breweries, &c.

Their construction is by no means expensive; they save iron-work, and require only bricks or tiles. These are recommended to be placed edgewise, and chosen as thin as possible for the inner walls. The circulating pipes are to be placed so, that rain falling down the chimney can never get into them. The method of using them is so easy, that in the largest public buildings one person is sufficient to light all the fires. All the wood that can be contained in the fire-place, which is very small, is to be put in at once; it is to be sawn into pieces of equal lengths; and as soon as it is burned, the slider that stops the communication of the circulating pipes with the chimney is to be thrust in. By these means all the heat, which the fuel is capable of producing, remains in the pipes, and issues out slowly, and only to diffuse itself in the apartment; while a single piece of wood, that had not burned at the same time with the rest, would oblige the slide to be left open, and the current of air necessary for its combustion would carry off into the chimney the greater part of the heat produced.

The following is a description of the stove constructed under my directions.

Fig. 1, Plate III. represents a front view of the stove: its height is 164 centimetres (about 61 inches French), exclusive of the vase, which is a separate ornament, merely placed on the top.

Its breadth is 85 centimetres (about $31\frac{1}{2}$ inches.)

Its depth 58 centimetres (about $21\frac{1}{2}$ inches.)

The height may vary according to the size of the apartment, and be extended without inconvenience to two metres (about 6 feet, 2 inches.) It may likewise be reduced, as I have done for stoves in a laboratory, which were to support a sand bath as high as the hand.

The other two dimensions are determined by those of the bricks employed to form the interior circulatory pipes, which should be in certain proportions, that the smoke may pass through

through them freely, without so much air entering with it as would condense it, or sink the temperature below the degree necessary for combustion.

V V are the external parts of the two heat openings. Heat openings.

m m Apertures of the stove, by which the air, that is to issue through the heat openings, enters. These are closed when the air is drawn from without through a pipe passing under the floor; which is much more advantageous for renewing the respirable air of the apartment, and prevents the danger of currents of cold air attracted by the fire; and which is necessary, as I have observed, whenever the volume of air in the apartment is not sufficient, to supply both the consumption of the fire, and the circulation in the heat pipes.

Fig. 2. is a plan of the foundation of the hearth at the height of the line A B, fig. 1. *ll* are empty spaces, to receive the air, and convey it into the compartments, where it is to be heated before it issues by the heat openings, whether the air be obtained from without, or simply by the apertures *m m*, fig. 1.

Fig. 3. pl. IV. plan at the height of the line C D, fig. 1; that above the door of the fire-place, *n n n n* are the double plates of cast iron, forming the compartments in which the air is to receive the effect of the heat of the fire,

o o The empty space between these plates.

Fig. 4. Front section at the line I K, fig. 3. The arrows indicate the direction of the smoke in the circulatory pipes of the front part.*

In this the plates of iron *n n* are seen in their perpendicular situation, with the tongues which form their compartments on each side of the fire-place. One of these plates is represented in front fig. 7.

T is an opening left at the bottom of the fourth circulatory pipe, to restore the draught of air in the fire-place, if neces-

* Among the number of Swedish stoves described and delineated in the collection published by baron Cronstedt there are several, the circulating pipes of which pass under the hearth. This gives them a little more extent no doubt, but as soon as the hearth is covered with ashes, the air passing beneath can receive but a very slight impression of heat; it obliges the fire-place to be raised higher; and it renders the construction more complex and expensive. For these reasons I have adopted the most simple plan.

fary, by burning there a few slips of paper, or other light combustible, I say *if necessary*, because I have found by experience, that this precaution may be neglected, as soon as the stove has been heated so as to have lost all its internal dampness.

The door of this sort of blower, or air-vent, ought to shut very close. For this purpose it is sufficient, to cut a piece of brick of the proper size, to make a hole in it to receive a handle, and to fasten upon it a piece of plate iron projecting a little all round it.

Fig. 5. Plan at the height of the line E F, fig. 1.

Fig. 6. Transverse section at the line G H of fig. 3, which shews the height of the fire-place, and the first direction of the flame.

V points out the arrangement of the heat pipes.

The dotted lines give the profile of the party walls, which form the four grand circulating pipes.

R the pipe which conveys the smoke from the circulatory pipes into the chimney, and in which is the register that cuts off the communication. It is a common stove tunnel of plate iron; but it would be better to use a substance more slowly conducting heat, as an earthen tube made on purpose, for that part in which the slider or stop plate acts.

The elbow made by this pipe to reach the chimney renders it unnecessary to repeat, that it is a point of the first importance for the body of the stove to be completely separate from the wall. That which I have described is 25 centimetres (about 9 inches) distant from the nearest point of the nich in which it is placed.

S is an elongation of the perpendicular pipe that enters into the chimney. It is intended to receive the water that might condense in the upper part, to prevent it from getting into the stove. The cap at the end of this elongation allows the pipe to be cleaned without taking it down.

The dotted lines forming the square space Q mark a place where a nich may be made, or a sort of little stove, as is done in some of the Swedish stoves, and would advantageously supply the place of the brick-work, with which it must otherwise be filled up.

All these figures being drawn on the same scale, there will be no difficulty in preserving the proportions of the parts.

The construction of this stove is neither difficult nor expensive. For the outside nothing is wanted but Dutch tiles, such as are used for common stoves, that is to say thin in the middle, and having a border all round, which serves to give them more stability. They are fixed in like manner by a band of metal. The hind part may consist entirely of bricks. The vase placed on the slab of marble or stone, which covers the stove is a mere ornament.

Materials of which the stove is constructed.

If it be thought proper to have no heat openings, all the interior structure may be made of bricks of proper sizes, laid with loamy earth moistened, and set on edge for the circulatory pipes, without any iron except a cast plate over the fire-place, and a door and frame in the usual manner.

The heat openings may be omitted.

The expence of the heat openings however consists only in four cast iron plates with tongues and grooves to form the compartments represented at fig. 7. All the rest is done with plate iron, bent round and rivetted, which, when once enclosed in the masonry, will not admit the escape of the air.

The expence however not great. How are they made.

Cast iron plates with grooves are well known, since Franklin's stoves have been adopted. If it were found difficult to procure them, their place might be supplied in two ways. First by portions of pipes of cast iron, which might be placed vertically side by side, serving as the inside walls of the fire-place, and communicating with each other by little channels at top and bottom formed in the masonry. Secondly, by common plain cast plates, soft enough to admit of being bored, so as to rivet on bent slips of plate iron, which would perfectly answer the purpose of the tongues and grooves. As these would never be exposed to the action of the flame, there is no reason to fear their casting. The latter of these two methods is obviously the most advantageous, as it occupies less room, and yet affords more surface to receive the action of the heat, and communicate it to the circulating air.

Substitutes for the cast-iron plates.

In concluding my description of this stove I ought not to omit saying, that nearly two years experience has convinced me of the good effects of its proportions.

Its utility confirmed by two years experience.

It is placed in a room fronting the north, the floor of which measures 47 metres square (about 12 toises $\frac{1}{2}$) and which is 42.5 decimetres (13 feet) high.

Account of its effects.

Every day a log of wood 23 or 30 centimetres (10 or 11 inches) round, sawn into three pieces, or an equal quantity of smaller wood, is burned in it at once. The slider of the door

of the fire-place is shut, and the key R, fig. 6 is turned, as soon as the wood is reduced to charcoal. Ten hours after the air throughout the room is at a temperature above the mean; and the centigrade thermometer, placed 36 centimetres (above 13 inches) from the stove, rises rapidly to 16 or 17 degrees.

To shew still more plainly to what degree the economy of fuel and preservation of heat may be carried by this construction, I shall relate another experiment, which I have repeated on several occasions, and which has always afforded me very nearly the same results.

The thermometer in the room, in which there was no fire the day before, being between 9 and 10 degrees, a log sawn in three as usual was put into the fire-place about eleven in the morning; and at three in the afternoon a similar quantity of fuel was put in.

At four o'clock the thermometer, placed at the distance above-mentioned, was at 42 degrees.

At five, at 37 degrees.

At seven, 34.

At nine, 31.

At midnight, 26.

You could not bear to touch with the hand the iron rim of the heat openings. The bulb of the thermometer being placed opposite one of these openings, at the distance of 8 centimetres (about 3 inches) rose in four minutes to 35°.

The next morning at 9 o'clock the thermometer, which had been again placed at the distance of 35 centimetres, was at 22°.

Finally at noon, that is to say twenty-one hours after the last wood was put in, and eighteen hours after the key had been turned, all the wood being reduced to charcoal, the thermometer stood between 18° and 19°. It was then placed two centimetres only from one of the heat openings, and in less than six minutes it rose to 26°.

These effects are so different from what we commonly obtain by the consumption of three or four times as much fuel, that I may expect more than one reader to suppose them exaggerated; but I hope a sufficient number will be found disposed to make a trial of these stoves, that their testimony and example may at length triumph over our habits, and produce a general conviction, that, without suffering any privation ourselves, we may preserve for our offspring what useless waste is daily robbing them of in an article of the first necessity.

VII. A Con-

VII.

A Continuation of the Experiments and Observations on the Light which is spontaneously emitted from various Bodies; with some Experiments and Observations on solar Light, when imbibed by CANTON's Phosphorus. By NATHANIEL HULME, M. D. F. R. S. and A. S.*

SECTION XI.

The Effects of various aerial Fluids on spontaneous Light.

INTRODUCTION.

THE apparatus employed for experiments with any kind of Apparatus made air, unless otherwise expressed, consisted of the following ^{use of,} parts: 1. A tea-faucer, holding about three ounces of water. 2. A wide-mouthed phial, which would contain about ten ounces of liquid. 3. A small wooden stand, composed of a slender pillar or pin, nearly four inches high, fixed into a round base, a little more than an inch in diameter, and half an inch thick. This stand was fastened by strong thread to the middle of a piece of flat lead, such as lines Chinese tea-chests, having holes in it to admit the thread; the lead was about three inches square, and doubled, to give it weight and stability. The top of the pillar was made pointed; and a round piece of cork, about an inch in diameter, and half an inch thick, was fixed upon it, by means of a superficial hole bored in its under part with a gimlet.

When the whole apparatus was put in use, the phial was ^{and its use.} filled with cold pump water, in a pneumatic tub, then inverted, and the species of air to be employed was let up into it, to the quantity of about eight ounces. The subject for experiment being applied to, or fastened upon, the top of the cork, the stand was placed on the tea-faucer, and then introduced, under water, into the phial containing the air. The whole apparatus, being now supported by the tea-faucer,

* Phil. Transf. 1801. p. 483. The former part is in the Phil. Transf. for 1800, page 161, and also in the Philos. Journal, Quarto, IV. 421, 451.

with

with water in it, was deposited in the laboratory for experiments on light. By this contrivance, the experiments were made in about eight ounces of air, by measure, confined above two ounces of water.

§ 1.

The Effects of common or atmospherical Air on spontaneous Light.

EXPERIMENTS.

The exclusion of atmospheric air prevents the appearance of spontaneous light.

Fig.

Experiment 1. Two fresh herrings were hung up together in the laboratory, so as to touch each other at their flat sides; and it was observed that the parts in contact remained dark, while those exposed to the open air became very luminous.

Exp. 2. Another fresh herring was laid upon a piece of thick brown paper, and placed in the laboratory. On examination, the next evening, the upper part, which was exposed to the air, was very lucid; but the underfide, lying upon the paper, remained quite dark.

Exp. 3. A luminous herring was divided transversely quite through its middle fleshy part; but the inside was perfectly dark. On the following night, that which before was dark had become luminous.

Exp. 4. At 9 P. M. a piece of fresh herring, of about three drams in weight, was introduced above water, into about eight ounces of atmospherical air. On the second night it was become luminous; on the third and fourth, it continued shining; and on the fifth the light was extinguished. This experiment was frequently repeated, with both the flesh of herring and of mackerel, and nearly with the same result.

Exp. 5. The cork of the apparatus was well smeared with the luminous matter of a mackerel, and then introduced above water. It continued to shine finely all that evening; and the light was not quite extinct on the succeeding night.

Exp. 6. Another cork was illuminated with herring-light, at half an hour past six P. M. and introduced above water. It remained very bright at eleven; and retained a glimmering light the next evening. The two last experiments were often repeated, and, in general, with similar effects. It may not, however,

however, be improper to observe, that the illumination of the cork did not always continue so long as twenty-four hours; for it must, of course, vary according to the quantity of luminous matter applied, and its degree of brilliancy.

Exp. 7. A large piece of rotten wood was received from Rotten wood. the country, which shone only in one place. The luminous portion was sawed off for use, and the dark part left in the laboratory. On going into the laboratory, the second night after this operation, I was surprised to see the dark piece, which had been left there, very lucid in several places where small splinters had been broken off in sawing; many shining fragments also lay scattered on the floor.

Exp. 8. A quantity of rotten wood, moderately shining, was blown upon for some time with a pair of bellows; but I could not perceive that this had any effect on the light, so as to render it more vivid.

Exp. 9. A small piece of shining wood was tied upon one of the corks of the apparatus, and introduced above water, where it continued lucid until the fifth night. In another experiment, the light was extinguished on the fourth night; and and in a third much sooner.

Exp. 10. A living glow-worm, in a shining state, was Glow-worm. submitted to the action of a pair of bellows; but the con- These are not tinuance of the blast did not apparently increase its glowing excited by bel- quality. lows.

Exp. 11. A very luminous dead glow-worm was fixed upon a cork of the apparatus, by means of a small pin, and then put into the phial, above water. It continued to shine as vividly as it did when in the open air, forming a pure white light, of a circular shape.

OBSERVATIONS.

Observation. 1. These experiments prove, that objects which abound with spontaneous light in a latent state, such as the herring, mackerel, and the like, do not emit it when deprived of life, except from such parts as have been some time in contact with the air.

Obs. 2. They likewise show, that the blast of a pair of bellows does not increase this species of light, as it does that which proceeds from combustion.

§ 2.

The Effects of oxygen Gas or vital Air on spontaneous Light.*

EXPERIMENTS.

Fish, shining wood and Glow-worms in common air and in oxygen gas exhibited no difference of illumination.

Experiment 1. A piece of fresh herring, of about three drams weight, was introduced above water, into eight ounces of oxygen gas. On the second night it was observed to be faintly luminous; on the third, the quantity of light was increased; on the fourth, it continued nearly in the same state; and on the fifth the light was diminished.

Exp. 2. A piece of very fresh mackerel, of the same magnitude, was also put above water. On the subsequent evening it was pretty lucid, and continued the same on the night following.

Exp. 3. At 9 P. M. a cork, finely illuminated with mackerel-light, was introduced above water: it continued very lucid at eleven. On the next evening it was dark.

Exp. 4. Another cork, rendered luminous with the same kind of light, was put above water at 10 P. M. The next morning, at six o'clock, only a glimmer of light was perceived and at 10 P. M. it was extinct.

Exp. 5. At 9 P. M. a fragment of shining wood was introduced above water; it was observed to be still very luminous at eleven; but the light was not quite so vivid, nor so extended in breadth, as when the wood was put in. On the succeeding night, at eight o'clock, it remained faintly lucid.

Exp. 6. A little after 8 P. M. another fragment of wood, shining very brightly, was introduced above water, into the same air that was used in the last experiment: it continued very luminous at eleven; but the light was diminished in quantity. On the next evening it was found to be extinguished.

Exp. 7. The same air was employed again at 8 P. M. with a pretty large and thick fragment of wood, uncommonly lucid: its light continued vivid and broad at half an hour past eleven. The following night, at eight o'clock, the light was still somewhat extensive and bright.

Exp. 8. In three other experiments with shining wood, in fresh oxygen gas, the light was totally extinguished in the space of twenty-four hours.

* The oxygen gas made use of was obtained from manganese, by means of heat.

Experiments were made, at the same time, and in the same manner, with atmospherical air and shining wood; but it was not very evident that the wood shone more vividly in the latter air than it did in the oxygen gas.

Exp. 9. A living glow-worm was put into a two-ounce phial, with a glass stopple, containing pure oxygen gas, and kept therein for some time. It was then taken out, and exposed to the open air; but no difference, either in the brilliancy or the quantity of its light, could be discovered.

Exp. 10. A luminous dead glow-worm was then inclosed in about five ounces of the gas; but no increase of its shining quality could be perceived.

Exp. 11. At six o'clock P. M. a shining dead glow-worm was introduced above water into oxygen gas: it continued very lucid therein at 7 P. M. shewing a pure white light. It was then taken out, and put above water into atmospherical air, where it shone, to all appearance, as splendidly as it did when it was in the oxygen gas.

OBSERVATION.

It appears, from these experiments, that oxygen gas does not act upon this kind of light, so as to render it much more vivid than it is in atmospherical air; which is quite contrary to what some authors have alledged.

§ 3

The Effects of azotic Gas on spontaneous Light.

1. *Azotic Gas, obtained from lean muscular Flesh and diluted nitric Acid, in a very low Heat, as recommended by M. de FOURCROY.*

EXPERIMENTS.

Experiment 1. A piece of fresh mackerel, weighing about three drams, was introduced above water, into about eight ounces of this azotic gas; and it was retained therein five days, without emitting any light.

Exp. 2. About the same quantity of fresh herring was then put above water, into the same gas used for the last experiment, and remained in it for the space of three days, in a dark state. This experiment was repeated, and with a similar result.

Exp. 3. At 45 minutes past 7 P. M. a cork, finely illuminated with mackerel-light, was put above water into the gas, and it was found pretty luminous at eleven. On the next evening, at eight o'clock, it still exhibited a faint degree of light.

Azote gas does not admit of the production of spontaneous light but maintains it in some instances for a time if previously produced.

A similar experiment was made, at the same time, in atmospheric air. At 11 P. M. the cork was but moderately luminous; and on the next evening it was dark.

Exp. 4. At 40 minutes past 7 P. M. another cork, rendered very luminous with herring-light, was introduced above water. This cork, at 11 P. M. was not found so lucid as that in the third experiment. On the next evening, a glimmer of light was still perceptible.

Exp. 5. A fragment of very shining wood was introduced above water, into this gas; and it was rendered dark in about 15 minutes.

Exp. 6. The experiment was repeated; and the light was again extinguished in about 15 minutes. In another experiment it was extinguished in about 25 minutes.

II. *Atmospheric Air rendered azotic, by burning Spirit of Wine in it, when confined above Water*

Atmospheric
azote.

Exp. 7. A portion of fresh herring, of about three drams, was put above water, into this azotic gas, at 5 P. M. On the second evening, a spark of light was observable; on the third, the quantity of light was increased; on the fourth, it was again diminished.

Exp. 8. At 3 P. M. the usual quantity of herring was introduced above water. On the second night, it remained dark; on the third it was moderately luminous; on the fourth, it was less so; on the fifth, the light was extinct.

Exp. 9. A piece of fresh mackerel was next put above water, at 11 A. M. On the second evening, it was found to be slightly luminous; it remained so on the third; on the fourth, it was dark.

Exp. 10. Another piece of fresh mackerel was introduced above water, at 3 P. M. On the second night, it was found to be slightly luminous; but on the third, it was dark; and no more light was emitted, though it was kept in the gas for the space of four days.

Exp. 11. A cork, made very luminous with herring-light, was put above water, into this gas, at 20 minutes past 8 P. M. and it continued very lucid at eleven. The next evening, at ten o'clock, the light was nearly extinguished.

A similar experiment was made, at the same time, in common atmospheric air, and with the same result.

Exp.

Exp. 12. Another cork was introduced above water, with herring-light, at 40 minutes past 7 P. M. and it remained pretty luminous at eleven. On the following night, it was nearly extinct.

III. *The last mentioned azotic Gas, after being washed with Lime Water.*

Exp. 13. A piece of herring, of about three drams weight, was put above water, into this azotic gas, at 5 P. M. On the second night, it was dark; on the third, very lucid; and on the fourth, the same.

Exp. 14. The experiment was repeated, on a piece of herring, at 3 P. M. On the second evening, it was dark; on the third, pretty luminous; on the fourth, it was less so; and on the fifth, only a faint light remained.

Exp. 15. A portion of fresh mackerel was then put above water, at 11 A. M. On the second night, it was observed to be moderately shining; on the third, the light was extinct.

Exp. 16. Another piece of fresh mackerel was introduced above water, at 3 P. M. On the second evening, it was slightly luminous; on the third, it was dark, and continued so during the four succeeding nights.

Exp. 17. A cork, finely illuminated with herring-light, was next introduced above water, into this gas, at 20 minutes past 8 P. M. The light was much diminished at 45 minutes past 8; at 11 the cork had become almost dark. On the following night, a glimmer was still apparent.

Exp. 18. Another cork, made very luminous with herring-light, was put above water, at 40 minutes past 7 P. M. and it continued pretty lucid at eleven. On the next evening, the light was merely visible.

A similar experiment was made, at the same time, in atmospheric air, and with nearly the same effect.

OBSERVATION.

It is a remarkable circumstance, that azotic gas, which is incapable of supporting light from combustion, should be so favourable to the spontaneous light which is emitted from fishes, as to preserve its existence and brilliancy for some time, when applied upon a cork; yet that it should prevent the flesh of the herring and the mackerel from becoming luminous, and also extinguish the light proceeding from rotten wood.

The

§ 4

The Effects of hydrogen Gas or inflammable Air on Spontaneous Light.*

EXPERIMENTS.

Hydrogen gas
does not admit
the production
of light.

Experiment 1. At 9 P. M. a piece of fresh herring, weighing about three drams, was introduced above water, into hydrogen gas. It was retained therein three days and three nights, without emitting any light. It was then taken out, and exposed to the action of atmospherical air. On the following night it was found to be luminous; but was dark again on the next night.

Exp. 2. Another piece of fresh herring was put above water, at 6 P. M. This was also kept in the gas the same length of time, without producing any light. It was then exposed to the open air, and inspected two successive nights, but it remained dark.

Exp. 3. The same experiment was then made with a piece of mackerel, which was taken out on the fourth night, without producing any shining appearance. The next evening, it emitted a very faint light, which did not continue twenty-four hours.

but extinguishes
it. (Fish)

Exp. 4. A cork, brilliantly illuminated with mackerel-light, was introduced above water; and the light was extinguished in about the space of an hour.

Exp. 5. At 39 minutes past 9 P. M. another luminous cork was put above water; it lost some of its light pretty soon, but was not extinct at twelve.

Exp. 6. A cork, with herring-light, was introduced above water, at 23 minutes past 6 P. M. The light gradually diminished, and was only faintly visible at eleven.

Exp. 7. A fragment of very shining wood was put above water, at 9 P. M. and was dark at eleven.

Common air
again revives it.

Exp. 8. Another fragment was put above water, at 40 minutes past 8 P. M. at 50 the light was much diminished, and at 8 minutes past 9 the shining ceased. The wood was then taken out, and exposed to the open air, when the light revived in a very beautiful manner.

* This gas was obtained from zinc and diluted sulphuric acid.

Exp.

Exp. 9. A piece of uncommonly shining wood was introduced above water, at 58 minutes past 8 P. M. it remained for a short time very luminous, but at 25 minutes past 9 the light was greatly diminished; at 20 past 10 it was nearly extinguished; and at 29 past 10 was quite dark. It was then exposed to atmospheric air, and the light revived very brightly.

Repetition of
extinction and
refuscitation.
(Wood)

Exp. 10. The same experiment was repeated, at 35 minutes past 8 P. M. the shining property was much diminished at 9; and at 10 it was very faint. The next evening, it continued merely visible. The wood was now taken out, and the light soon revived very strongly. The following night, it was still moderately lucid; but on the next evening nearly extinct.

Exp. 11. Finding, by the above experiments, that the light of shining wood was extinguished by this species of gas, and restored by atmospheric air, the following three trials were made, to discover, in some degree, how long its light might be kept in a latent state, and then be revived. At 9 P. M. several fragments of shining wood, tied up in a piece of gauze, were introduced above water, into the hydrogen gas, and the light was gradually extinguished during that evening. They were kept there in that dark state 48 hours, were then taken out, and exposed to the open air, when, after a little time, the light re-appeared.

Extraction for
48 hours.

Exp. 12. On the 2d of October, another fragment of exceedingly shining wood, two inches and a half long, and pretty thick, was put above water in the evening, and its light was gradually extinguished. On the second night, it was taken out perfectly dark, but its light recovered by degrees, and became brilliant. It was introduced again, that evening, into the same gas, and its light disappeared. On the third night, it was again exposed to the open air, and the light revived as before. It was then reinstated and extinguished, and continued in a dark state, from the third to the fifth night, when, being again taken out, it soon shone in a pretty vivid manner. It was again introduced and extinguished as usual; and no observation was made of it, from some accidental circumstance or other, until the 10th of November in the evening, when it was taken out, and exposed to the open air for a length of time, but the light did not revive.

Three repetitions
of extinction
and renovation.

Exp.

No renovation
after a week

Exp. 13. A third fragment, somewhat larger than the former, and equally luminous, was put above water, at the same time as the one in the last experiment, where it was soon deprived of its light. It was retained there, in a dark state, from the 2d of October till the 10th of November; it was then taken out, and exposed to the action of atmospheric air, for several days, but there was no return of light.

Three extinc-
tions and rene-
vations of a
glow-worm

Exp. 14. About 7 P. M. a shining dead glow-worm was introduced above water into the gas, and its light was soon extinct. It was then exposed to the open air, where, in a very short time, it shone as brightly as before.

Exp. 15. At half an hour past 9 P. M. the same glow-worm was again introduced above water; when its light in a short time disappeared. It was taken out for exposure to common air, at 11, and its glowing property was immediately restored. It was again replaced in the gas, where it soon lost all its light a second time, and was kept in that dark state for 24 hours; when taken out, it continued dark for a little time, and then the insect gradually recovered its pristine splendour.

OBSERVATION.

From these experiments we learn, that hydrogen gas, in general, prevents the emission of spontaneous light, and also extinguishes it when emitted; but, at the same time, it does not hinder its quick revival, when the subject of the experiment is again exposed to the action of atmospheric air; although the light may have been a considerable time in an extinguished state.

(To be concluded in our next.)

Caution

VIII.

*Caution against the great Danger of keeping Phosphorus in Bottles without particular Caution. By DESCROZILLES the Elder.**

I have narrowly escaped falling a victim to an accident occasioned by the effect of the frost upon a bottle, which contained a hectogramme of phosphorus, with a quantity of water sufficient for covering this highly inflammable substance. This morning, before day-light, some books and a chest suddenly caught fire in the apartment in which I lay, and which is not occupied during the night. It was still fortunate that the two hours of my accustomed sleep were long since elapsed. Some seconds later, I should have been suffocated by the deleterious vapours of the phosphoric acid. Quickly gaining the door, I called for assistance, and we succeeded in extinguishing the fire before it had made any progress.

Dangerous
accident of fire
from phosphorus.

According to all appearance, the frost which had taken place some days before in this piece, had caused the bottle to break; but it was surrounded with paper, which prevented its falling to pieces. By this means, in proportion as the water ran off by the effect of the thaw, the bundle of phosphorus, exposed to the atmospheric air, was situated under circumstances the most favourable to ignition. The sides of the bottle performed the office of a small furnace, in which the cylinders of the combustible were propped against one another. Soon the gradual combustion which produces phosphorus acid was succeeded by the rapid deflagration, the result of which is phosphoric acid.

How it happened.

Independently of the effects of frost and of blows, a bottle frequently breaks without any apparent cause, and as it were spontaneously: It appears therefore to me, that in order to obviate an inconvenience which in some cases may prove very serious, the best means would be to use cases of copper, strongly foldered, and lined internally with paper or bran, for inclosing the bottles filled with phosphorus and water. Cases of tinned iron would be destroyed much sooner by oxidation, and their foldering would be susceptible of detaching itself by the effect of a moderate heat.

Other cautions.

Finally, it appears to me that this report ought to be as public as possible.

* In a letter to the Editors of the *Annales de Chimie*, No. 123.

IX.

Observations in Answer to DR. PRIESTLEY'S Memoir in Defence of the Doctrine of Phlogiston. In a Letter from MR. WILLIAM CRUICKSHANK.*

To Mr. NICHOLSON.

SIR,

Woolwich, March 22, 1802.

Observations of Dr. Priestley, that metallic calces contain little addition except water.

But when reduced by mere heat they afford pure oxygen, very nearly equal to their loss of weight, and no water.

If calces contain water only, then charcoal must produce the same

IN your Journal published the 1st of this month, I find a letter from Dr. Priestley, in which he still defends the old doctrine of phlogiston, but with very little success; for having additional difficulties to struggle with, he has been under the necessity of adopting new, and sometimes contradictory opinions, in his explanations and defence; For example, it is now his opinion that all metallic calces contain water and little or nothing else; and that charcoal, uniting with water, forms both fixed and inflammable air; for as this substance contains the elements of both kinds, nothing but water is wanted to enable them to take the form of air. It is almost impossible to argue against such strange suppositions and loose reasonings as these. However, let us suppose water to be the only substance contained in oxides; if so, heat alone ought to revive at least some of them, and in this case nothing but water should be separated. Now that heat alone revives several, particularly those of mercury and the perfect metals, is a fact sufficiently ascertained; but, instead of water, we obtain the purest oxygen gas, the quantity of which added to the revived metal, amounts, as nearly as possible, to the weight of the original oxide; even from the ore of manganese, so difficult to reduce, a prodigious quantity of the purest oxygen gas may be procured by a moderate heat; and when the oxide has been previously reduced to powder and well dried, by a heat nearly red, no water whatever can be perceived: in this case, too, the quantity of the oxygen gas disengaged, will be found to correspond very nearly with the loss of weight in the oxide.

How is it possible, then, that these phenomena can be explained on the supposition, "That all the calces contain little

* Philof. Journal I. 181,

"or nothing else but water?" when not a particle of water ^{thing by heat} is to be seen during the revival, or partial revival, of the me- ^{with calx as with} tals from their oxides in close vessels. Let us, however, con- ^{water;} sider this supposition in another point of view; which is, that if water only were contained in these oxides, then the gas obtained from a mixture of them and charcoal, should be the very same as that procured from moistened charcoal; but the con- ^{contrary to fact.} trary of this I have clearly proved to be the case (see Phil. Journal, vol. v. quarto, p. 6.); and this was one of the principal faults which led to the discovery of the gaseous oxide.

I shall here just enumerate a few of the properties by which ^{Enumeration of} these gases may be readily distinguished, being deduced from a ^{differences; spe-} number of experiments, often repeated with uniform results. ^{cific gravity of} First, then, the specific gravity of the gaseous oxide of carbon ^{gaseous oxide of} is no less than double that of the gas obtained from moistened ^{carbon double} charcoal, being as 30 to 14.5. Secondly, The proportion of ^{that of hydro-} oxygen necessary to saturate the gaseous oxide, is to that re- ^{carbonate;} quired by the hydrocarbonate as 15 to 44.8, or 1 to 3, nearly ^{saturation oxygen} estimating by quantity. And, thirdly, which is the most dis- ^{very different;} tinguishing property of the whole, the same quantity of oxygen, ^{and products of} suppose 14 parts, which, when combined with the gaseous ox- ^{carbonic acid} ide of carbon, produces from 36 to 44 parts of carbonic acid ^{with like por-} (according to the purity of the oxide), will, when combined to ^{tions of oxygen.} saturation with the hydrocarbonate, produce only 12 parts of the same acid, accompanied, however, with much water, proceeding in part from the hydrogen in the charcoal, of which all common charcoal, it would appear, contain a certain proportion (see P. Jour. No. 55. p. 210 and 211, and also the Table of Analysis, &c. p. 8. No. 59.) Surely gases having properties so essentially different, can never be considered as the same.

Dr. Priestley remarks, that there is a considerable difference ^{Differences in} in the qualities of heavy inflammable air, depending not only ^{heavy inflam-} on the substance employed, but also on the stages or periods of ^{mable air noticed} the process itself*. Now I have found that these variations ^{by Dr. Priestley,} never take place in any remarkable degree but when charcoal ^{are found only} is employed in some form, and that even then the differences ^{when charcoal is} chiefly depend upon the imperfect state of the charcoal; for if ^{present,} good charcoal be exposed to a red heat in close vessels during ^{and more or less} 10 or 15 minutes, and then mixed whilst hot with the substance ^{impure;}

* P. Journ. No. 3. (O&.) p. 183.

to be employed, likewise hot and completely dried, the variations in the qualities of the gases will be much less, the principal difference depending upon the proportions of the carbonic acid and the inflammable gases; but the gases from pure charcoal, either alone or moistened, never have any of the distinguishing characters of the gaseous oxide, being always much lighter, yielding bulk for bulk when saturated with oxygen, not more than one-third of the carbonic acid gas afforded by the

the hydrocarbonates are much lighter than the gaseous oxide, and they afford water by combustion with oxygen.

Erroneous quotation and remarks.

other. What, however, distinguishes them still more completely, is the large proportion of water generated by the combustion of the hydrocarbonate in oxygen gas; for the gaseous oxide when pure, or burned under the same circumstances, never produces the least sensible quantity of this fluid.

In mentioning the circumstances of the production of the gaseous oxide of carbon from the metallic calces and charcoal, &c. Dr. Priestley has somehow mis-stated both the meaning and words in what he calls a quotation from the first paper; this passage in his letter is as follows (see p. 182.): "After repeating my experiment, which he found to be just, Mr. Cruickshank did the same with the calces of the other metals, as zinc, copper, &c." and then concludes (p. 4.) "*that in ALL these cases the air must come from the partial decomposition of the carbonic acid by the calx, when raised to a high temperature.*" Then the Dr. goes on and adds: "But the inference that I think is more naturally drawn from them is, that all these calces contain much water, and little or nothing else." Now the passage from which this quotation appears to have been taken (for there is nothing in p. 4. exactly similar to it), does not follow the account of the experiments with the calces of the metals, &c. but is an inference drawn from the first experiments made with the carbonates and the iron scales. I shall insert this passage, and leave the philosophical reader to judge for himself of the accuracy of the statement and the justness of the conclusion. "Conceiving that in these experiments * the gaseous oxide must proceed from the partial decomposition of the carbonic acid by the iron when raised to a high temperature, I thought I should succeed better by employing iron filings in place of the grey

* Alluding to these made with the carbonate of barytes and iron scales, or imperfect grey oxide.

"oxide, as these would have a greater affinity for oxygen."

From the above statement the misrepresentation both of the explanation and words must be manifest, and the insertion of the word *all* is curious enough. (Essay on Gaseous Oxide, &c. Philos. Journal, vol. v. quarto, p. 4.)

In the experiments made with the mixtures of carbonate of lime dried as much as possible and metallic filings, the Dr. conceives that the gas must have been produced by the water which, still remaining in the carbonate, had united with the phlogiston of the metal, and passed over in the form of inflammable gas, mixed with the fixed air separated from the chalk.

If the inflammable gases were produced in these cases by the water separating from the earth, and passing over the red-hot metal in the form of steam, then they should be the very same as that obtained by passing the vapour of water through a red-hot iron tube; but it is well known, the gases thus produced are so far from being the same, that they are extremely different in all their properties; for the hydrogen, or light inflammable air, produced by the decomposition of the water in passing through the tube, is the lightest of all aeriform fluids, and, when combined with oxygen, there is not the least appearance of carbonic acid, and nothing formed but water. On the contrary, the gaseous oxide procured by heat from a mixture of the driest earthy carbonates with metallic filings, is the heaviest of all known inflammable gases, and when united with oxygen, produces nothing but carbonic acid, there not being the least appearance of water. It is impossible, therefore, that these heavy inflammable gases should be produced by water alone, in any state, acting upon pure metals; for the gases obtained from the decomposition of this fluid, whether from the solution of metals in dilute acids, or from their action on its vapour when raised to a red heat, are always of the same nature, being pure hydrogenous gas.

Dr. Priestley observes, that before I admitted that the iron or its calx, raised to a high temperature, could decompose the carbonic acid in this experiment, I should have tried whether it would do it in any other. This remark clearly proves that he had not seen the second essay on the gaseous oxide, &c. in your Journal for the month of August, in which a process is described, where this acid is decomposed, even in its gaseous form, by passing it repeatedly through a red-hot iron tube filled

filings and chalk heated, supposed by Dr. Priestley to afford a gas consisting of phlogiston and water.

If so, the gas ought to be the same as with filings and water;

but this is the lightest of elastic fluids, and forms water by combustion;

whereas the gaseous oxide is the heaviest inflammable gas, and produces carbonic gas by combustion, with no water.

Recapitulation of the experiment wherein carbonic acid gas passed over ignited iron gave oxygen to the metal, and was itself converted into carbonic oxide.

in

in the middle with iron wire. Bladders filled with this gas were attached, by stopcocks, to the extremities of the tube. After the middle of the tube, placed in a furnace, had been made red hot, the gas was repeatedly passed, and very slowly, from one bladder to the other. During this passage it was exposed to a very extensive surface of red-hot iron; and in one of the last experiments made in this manner, after forcing the gas through the tube backwards and forwards 30 times, four parts in five of the carbonic acid gas was converted into gaseous oxide: the gas upon the whole was a little diminished. The iron wires, after the operation, were covered with the same shining crust as if the steam of water had passed over them; and in fact they were oxidated or calcined to a certain extent, in consequence of seizing from the carbonic acid a proportion of its oxygen sufficient to convert it into the intermediate state of an oxide*.

Dr. P. could not decompose carbonic acid by the burning glass with iron, because the contact of ignited surface was insufficient.

Simpler method of decomposing carbonic acid gas.

Though mere heat does not expel oxygen from finery cinder; yet heat and the attraction of charcoal may;

The method by which Dr. Priestley attempted to decompose the carbonic acid gas, by heating pieces of iron with a lens placed in it, could not possibly have succeeded, at least to any sensible degree; for the heated air when left at liberty to ascend, would not remain in contact with it, nor even near it, for a moment: thus every portion of air, being unconfined, would expand prodigiously, so that very little of it could come or remain in contact with the heated metal. In the experiment above related, the gas was computed to pass several times over a surface of red-hot iron of some extent; yet, notwithstanding this, a considerable time was required to produce any remarkable decomposition in the acid; a circumstance which proves that, in the method employed by Dr. Priestley, no sensible decomposition could have been produced. There is likewise, in my second Essay, another method still simpler for decomposing the carbonic acid in its gaseous state†. All the oxidizable metals are, when raised to a high temperature, capable of decomposing it more or less; but zinc is by far the most powerful, owing no doubt to its greater affinity to oxygen. Dr. Priestley asks, if, in the experiment with the finery cinder and charcoal, the oxygen to form the carbonic acid should come from the calx, how is it to be expelled, as heat alone will not

* For the particulars of this experiment, and the mode of conducting it, see this Journal, quarto s. No. 55. p. 209.

† No. 55. p. 209.

do it, &c. &c.? But here he has forgotten that the affinities of bodies are remarkably varied by change of temperature, and that, in all reductions of metallic oxides, the carbon of the charcoal, when raised to a high temperature, unites with the oxygen of the calx in consequence of increased affinity, and forms with it carbonic acid gas and gaseous oxide. That this is the case is remarkably proved by distilling the red oxides of mercury and lead with charcoal; for these oxides we know contain a large proportion of oxygen, which may be separated by heat alone, but when heated with charcoal, nothing but carbonic acid and gaseous oxide come over, because, in this case, the whole of the oxygen being in its nascent state, combines, even at a low temperature, with the charcoal, and passes over in the form of carbonic acid gas mixed with the gaseous oxide. There are some circumstances accompanying the distillation of charcoal with these metallic oxides, which are easily reduced, and at low temperatures, not altogether uninstruc-
 as it does in all reductions.
 Red oxides of mercury and lead afford carbonic acid and oxide.
 Circumstances of these experiments.
 Deductions.

and, in my opinion, unanswerable by the supporters of phlogiston. In these processes the proportion of gaseous oxide to carbonic acid gas is but small, being in general about one-third or one-fifth of the whole, and for the most part is obtained just before or at the time the mixture becomes red: It is always accompanied with a torrent of carbonic acid gas. At the instant the metal is revived, the gas either ceases entirely, or comes over very slowly; but on increasing the heat, it again makes its appearance, and is now so far from containing gaseous oxide, that it is peculiarly light, not mixed with any sensible quantity of carbonic acid gas, and yields, when saturated with oxygen, but a very small proportion of this acid gas.

The following facts may be drawn from these experiments: First, It would appear that a much greater degree of heat is necessary for the proper production of the gaseous oxide than for the carbonic acid. Secondly, That oxygen, in its nascent state, may unite with carbon at less than a red heat, and form carbonic acid; as is clearly proved by the process with the red oxide of mercury and charcoal. The gaseous oxide appears likewise to be produced at a very low heat.

I have now taken a view of all the arguments which Dr. Priestley has brought forward in defence of his former opinions, but shall at present make no further observation on this subject, leaving the argument and fact to be decided by your philosophical and chemical readers.—I am, SIR, &c.

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X.

On the new Planet CERES.

To Mr. NICHOLSON.

SIR,

Parson's Green, April 3, 1802.

The subject resumed.

AT the conclusion of the memoir concerning the new planet Ceres, which you did me the honour to publish in your last number of the Philosophical Journal, a want of leisure, and the length of the communication, were alledged as reasons for my not concluding, at that time, the whole of the observations which I had to offer on the subject: I beg leave, therefore, now to resume the examination and detail of those particulars which remain yet to be treated of.

Discovery of a method of deducing the greatest equation from the eccentricity.

About four years ago, when I was inventing a mechanical contrivance, by which the equation of the center and true distance of a planet, or any number of planets, might be exhibited in an orrery, I discovered that the *natural sine of half the greatest equation of any planet, is equal, or very nearly equal, to the decimal figures which represent the value of a vulgar fraction, composed of the eccentricity and mean distance of that planet*: For instance, if we take the mean distance of the earth from the sun at 100000, and the eccentricity, according to Lalande, at 1681,395, the fraction $\frac{1681,395}{100000}$, converted into a decimal expression of the same value, is 01681395; and, omitting the decimal point and three last figures, we shall have 01681 for the natural sine of $0^{\circ} 57' 47,6''$, which arc differs only about *half a second* from one half of the greatest equation, as given in the tables of the third edition of Lalande's Astronomy.

The process, in the form of an analogy, will be thus: As the mean distance : is to unity :: so is the eccentricity : to the natural sine of $\frac{1}{2}$ the greatest equation.

Tabulated numbers to shew its correspondence in all the planets.

This analogy will apply to all the other planets, as may be seen in the subjoined table, which I have calculated from the mean distances and eccentricities given in Lalande's Astronomy, and copied by Mr. Vince, except in the instance of Ceres, the data of which planet are taken from the elements of Gauss.

Planets,

Planets.	Vulgar Fractions.	Decimals, or Nat. Sines.	Correspondent Arcs.	Half the greatest Equation, 1750.
Mercury	$\frac{7255.4}{38713}$,20551	11 51 34,9	11 50 0
Venus	$\frac{498}{72333.24}$,00688	0 23 38,3	0 23 40
Earth	$\frac{1681.395}{100000}$,01681	0 57 47,6	0 57 48,2
Mars	$\frac{14183.7}{152769.27}$,09308	5 20 26,9	5 20 20
Ceres	$\frac{8250}{276710}$,02981	1 42 28,96	
Jupiter	$\frac{250137}{320179.2}$,04807	2 45 18,6	2 45 19,15
Saturn	$\frac{53640.42}{954072.4}$,05622	3 13 22,7	3 13 21
Georgian	$\frac{90804}{1918352}$,04733	2 42 45,5	2 43 38

In this table the greatest difference between the arcs contained in the same line of the two last columns, is that in the line of Mercury; but it may be worthy of remark, that half the greatest equation of this planet, according to Dr. Halley's tables, is $11^{\circ} 51' 18''$; and also that Lalande himself made a new determination of the elements of Mercury's orbit, as related in the "Memoires de l'Institute Nationale" of Paris for the "fourth year of the Republic;" in which the grand equation is given $23^{\circ} 40' 43''$.

Remark concerning mercury.

Here, then, it appears, that the greatest equation of the new planet Ceres, which corresponds to the eccentricity assigned by Gauss, and copied into the different Journals, is about $3^{\circ} 25'$, instead of $9^{\circ} 27' 41''$; so that, as I hinted before, either the eccentricity is almost two thirds *too little*, or the greatest equation almost two thirds *too much*. I mean not at present to enter into a geometrical demonstration of the analogy which I have used in procuring the above tabulated results; but leave it to exercise the ingenuity of your mathematical readers, some of whom will probably be induced to favour you and the public with a demonstration, as a separate article. I will, however, just prove to the reader the accuracy of the inference I have made with respect to Ceres, by means of the *elliptic hypothesis* of Ward, which is generally allowed to be a convenient approximation to be used for finding the equation of a planet, instead of either the direct or tentative methods, which are more accurate, but much more intricate.

Hence the greatest equation of Ceres, as assigned, does not correspond with its eccentricity.

Proof of the accuracy of this inference, by computation on the elliptic hypothesis of Ward.

By the elliptic hypothesis, the analogy for converting mean into equated anomaly is simply this: *As the aphelion distance : is to the perihelion distance :: so is the tangent of half the mean anomaly : to the tangent of half the equated anomaly*; and the difference between these two anomalies constitutes the equation itself. Now, it is well known to all who are conversant in the theory of planetary motion, that in the projection of any elliptic orbit, a circle, described from the focus in which the sun is supposed to be, with a radius that is a mean proportional between the major and minor semi-axes, will cut the ellipse in two points, which shall be the points of mean distance; or, which is the same thing, the points where the equation becomes stationary, and consequently where it is a maximum. It is also equally well known to practical astronomers, and calculators of an ephemeris, that the equation varies very slowly for many degrees both before and after the points of mean anomaly corresponding to the greatest equation; and likewise that these points fall a little beyond the first quadrant from the aphelion, or three degrees of mean anomaly, by a quantity which depends upon the eccentricity of the orbit. In the orbit of Mercury the point of mean anomaly, when the equation is greatest, is nearly at 105° from the aphelion; in that of Venus it is between 90° and 91° ; in that of the Earth about 91° ; in that of Mars about 97° ; in that of Jupiter and Georgian between 93° and 94° ; and in that of Saturn about 94° . Hence it may be inferred, that if the greatest equation of Ceres be $3^\circ 25'$, the said point of mean anomaly will be about 92° : but that if the equation be $9^\circ 27' 41''$, it will be about 96° ; namely, somewhat short of that of Mars, the greatest equation of which is $10^\circ 40' 40''$.

Let us try now what the greatest equation will be upon both suppositions successively, according to the *simple elliptic hypothesis*.

	Log.
As the aphelion distance (27673+825) 28498	4,45481
Is to the perihelion dist. (27673-825) 26848	4,42891
So is the tangent of 46° ($\frac{92^\circ}{2}$) $\frac{1}{2}$ mean anom.	10,01516
	<hr/>
	14,44407
	4,45481
	<hr/>
To the tangent of $\frac{1}{2}$ eq. anom. $44^\circ 17'$ nearly	9,98926
Then $92^\circ - 88^\circ 34' = 3^\circ 26'$ is the greatest equation.	

Again,

Again, supposing the point of mean distance to be at 96° , we have in that case,

As the aphelion distance, 28498 - - - - - 4,45481
Is to the perihelion dist. 26848 - - - - - 4,42891
So is the tangent of 48° - - - - - 10,04556

14,47447

4,45481

To the tangent of $46^\circ 17'$ nearly - - - - - 10,01966

Then $96^\circ - 82^\circ 34' = 3^\circ 26'$ is the greatest equation, as before.

Hence it is indubitably proved, that the equation, as given by Gauss, is much too great for the eccentricity; and it appears also, according to what has been already asserted, that the equation at 92° and 96° of mean anomaly is nearly the same; that is to say, the difference will only be in the seconds.

But the greatest equation of a planet is usually determined from a series of observations antecedently to the calculation of the eccentricity: therefore the error which has been detected may be in the eccentricity, and not in the equation; in which case, by reversing the analogy already used, we shall have this calculation, viz. As unity : mean distance 27673 :: natural sine of $4^\circ 43' 50\frac{1}{2}''$ or 08247,4 : 2282,2 for the requisite eccentricity. But it will be most easy to determine in which of the two elements the error has been committed, when the whole period has been accurately ascertained.

The equation given by Gauss is too great for the eccentricity;

or the eccentricity is faulty :

but this will be best known from the period, when determined.

When it was mentioned in the former paper on this subject, that oppositions and conjunctions were of importance to be observed, the reason was omitted to be explained; which is, that when a superior planet is in opposition, or an inferior one in conjunction, the observed geocentric longitudes are also heliocentric longitudes, without calculation or reference to distance and eccentricity; because in such relative situations there is no parallax of the orb: and it is well known to astronomers, that when an opposition happens at the place of mean distance of a superior planet, half the difference between the heliocentric place, by calculation of mean motion, and of the place as observed at that time, is equal to the greatest equation. The 13th of March ult. was the day on which the astronomers on the continent predicted that an opposition of Ceres would

Observations of conjunctions and oppositions, and their use.

occur; but it must have happened on the 23d, as I calculate from Von Zach's little ephemeris continued forwards; viz. when the geocentric plane was about 182° . The astronomer who has an observatory, and has noted the *exact time*, will do well to make the observation public.

Application to
the planet Ceres.

The mean time which elapses between two successive oppositions or conjunctions of a planet, as seen from the earth, is called a synodic revolution, and is determined by dividing 360° by the difference of the mean daily motions of the earth and other planets. Thus: taking the mean daily motion of Ceres at $770,7376''$, according to Gauss, and of the earth at $58' 8,33''$, according to Lalande, we have $\frac{360^{\circ}}{2777,3924} = 466,6$ days nearly for the whole synodic period, on a supposition that the motions are both equable throughout their orbits; but their respective distances from their aphelia at the time of opposition must be made the argument of a correction, either additive or subtractive, as the case may be, to determine what a synodic period would be if both motions were equable. Now, if we reverse this process, we can just as easily gain the difference of the daily motions between that of the earth and other planets, and consequently the whole period of the latter, from having only the earth's daily motion, and *observed synodic period*; for 360° , divided by this period in days, gives the difference wanted at once, which, subtracted from the daily motion of the earth, gives that of the other, if it be a superior planet; but if an inferior one, that difference must be added; and the more nearly the two daily motions approximate to each other, the longer will be the respective synodic revolution. In the instance before us, if we suppose the whole corrected synodic revolution of Ceres to be 466,6 days from observation, we shall have $\frac{360^{\circ}}{466,6} = 2777,3924''$ for the *difference* to be subtracted from $3548,33''$ the earth's mean daily motion, which will leave $770,7376''$ for the mean daily motion of Ceres, as before; by which if we divide 360° , we shall have the whole tropical period $= 1681^d 12^h 8^m 49^s$. But it remains to be observed what a whole synodic period of Ceres *may prove in reality*.

Determination
of the real pe-
riod, &c.

Supposing the epoch, or mean heliocentric longitude of Ceres to have been $2 S. 17^{\circ} 36' 34''$ on January 1, 1801, the day of its discovery, as is stated by Gauss, and the place of the aphelion $10 S. 26^{\circ} 27' 38''$, the mean anomaly must, on
this

this supposition, have been at that time $3^{\circ} 21' 8'' 56''$, so that it had passed the place of mean motion about either 19° or 15° , accordingly as we make the greatest equation $3^{\circ} 25'$ or $9^{\circ} 27' 41''$: therefore the daily motion was nearer a mean motion than it has been ever since; and it will be yet some months before it arrives at its place of mean motion in the opposite half of its orbit; which place is either 2° or 6° short of the ninth sign of anomaly, accordingly as we take the eccentricity. Let us suppose now the whole period to be upwards of 1681 days, as has been, perhaps prematurely, determined; one fourth of this time had elapsed on the 24th of February last; on which supposition, the *mean anomaly* must then have been advanced just *three signs* from the original situation; namely, it must have been upwards of $6^{\circ} 21'$, at which rate the planet had passed the perihelion by a space of time answering to 21° of mean motion, which is about 98 days: therefore the 18th of November, 1801, must have been the day on which it was at the perihelion, or place of greatest velocity; but at that time the planet *was lost*, and we are not in possession of any observation of it nearer that time than the 7th of December following, when Baron Von Zach re-discovered it.

The continuance of any planet in the first quadrant from aphelion is longer than in the second quadrant, by a space of time which corresponds to the whole equation, taken at three signs of mean anomaly; in which situation, it has been already observed, that the equated or apparent motion is also, as nearly as may be, a mean motion; if therefore the equation at three signs be divided by the mean daily rate of motion, we shall have a space of time, which, added to one fourth of the whole period, and subtracted from another fourth, will give nearly the respective times of continuance in the first and second quadrants of anomaly: Hence arises this rule for finding the two semicircles, respectively bisected by the perihelion and aphelion points, viz. divide four times the equation at three signs of anomaly, (which may be the greatest equation where the eccentricity is small), by the mean daily motion, and the quotient will be the number of days that the planet continues longer in the semicircle from nine to three signs of anomaly than from three to nine. For instance, if we take the equation

tion of Ceres at $3^{\circ} 25'$, we shall have $\frac{3^{\circ} 25' \times 4}{77^{\circ} 7376''} = 63,83$ days for the time of continuance in the semicircle embracing the aphelion, longer than in the semicircle which is bisected by the perihelion: but if we take the equation at three signs $= 9^{\circ} 25'$, somewhat less than the greatest equation, in this case, by reason of the increased eccentricity, we shall have the excess of continuance $\frac{9^{\circ} 25' \times 4}{77^{\circ} 7376''} = 175,93$ days.

Observations from which the position of the apsidæ are deduced, &c. &c.

This suggestion may be worthy the notice of the practical astronomer; for when a variety of observations are taken of the new planet in the different quadrants of its orbit, and the corresponding times recorded, it will be no difficult task, when equidistant geocentric longitudes are converted into heliocentric longitudes, to observe what semicircle of the ecliptic corresponds to that half of the orbit in which the planet has *continued longest*: the middle of that semicircle will be the *aphelion*, and the two extremities will be three and nine signs of anomaly: Also, the *excess of duration*, above the time occupied by the other semicircle, multiplied by the *mean daily motion*, will be *four times* the equation at three and nine signs of anomaly very nearly; and as this equation is very little short of the greatest equation, the eccentricity may likewise be found by either of the methods already described: Thus the form and elementary points of the orbit may be gained by a series of observations converted into heliocentric places, even by the projection proposed in the last memoir on this subject, and these determinations may be corrected by a comparison of them with the results deduced from the properties of an ellipse, which are here purposely omitted, lest a more minute and scientific description of intricate calculations should rather puzzle than inform the generality of readers*.

Inclination of the orbit, and place of nodes.

It remains yet that some observations be made relative to the *position* of the orbit of a planet. There are many methods of ascertaining the nodes of a planet's orbit, from calculation grounded upon observations; but the simplest, when it is practicable, is to convert the geocentric into the heliocentric place at the time when there is no latitude by observation, for

* See Lalande's and Mr. Vince's Astronomy; and also Professor Robison on the Geo. Sidus, in the Edin. Transf. vol. I. 1788.

the heliocentric place will be the place of the node, ascending or descending, as the case may be, which will appear by a subsequent observation; but when the place of a planet, when crossing the ecliptic, cannot be observed, the middle point between two equal north and south latitudes, gained by observation, will give the node.

The heliocentric latitude, when a planet is just 90° from each node, is the measure of the inclination of its orbit, and is easily obtained from the observed geocentric latitude, taken in that situation, by the analogy already described; or, otherwise, the greatest heliocentric latitude may be acquired from an observation of a geocentric latitude and longitudinal distance from the node, thus: When the earth is in the line of the nodes, the analogy will be, *as the sine of the difference of the longitudes of the sun and planet seen from the earth : radius :: tangent of the geocentric latitude : tangent of the inclination.*

The two days on which the earth will be in the line of the nodes of Ceres will be June 12. and December 13. this year. But it is beyond the proposed intention of this popular memoir to enter into all the minutiae of calculation, were the requisite data before me; but only to point out the *methods* of applying observations for determining the size, form, and position of a planet's orbit: it may not, however, be unworthy of notice, before I conclude, to remark, that the astronomers on the continent, who availed themselves of the earliest observations *only* for determining an approximate set of elements of Ceres, were enabled to do this from noticing that this planet became *stationary* between the 10th and 11th of January, 1801, when its elongation was known by observations; for it has been shewn by writers on astronomy, that, upon a supposition of circular orbits, *the tangent of the elongation is equal to the semi-diameter of the orbit, divided by the square root of that semi-diameter + 1.*

Days when the earth will be in the line of the nodes of Ceres.

The early determinations respecting the orbit of Ceres were made from its stationary position.

Your's, &c.

W. P.

APPENDIX.

April 10, 1802.

Since the preceding paper on the planet Ceres was written, Mr. Ed. Troughton has put into my hands the duplicate of another letter, sent to him by the Baron de Zach, and addressed to Sir Joseph Banks, Bart. which I understand has been read at a meeting of the Royal Society, and which I shall here transcribe*, on account of some remarks which I have to make upon it. (Copy,)

Seeberg Observatory near Gotha, Feb. 20, 1802.

" DEAR SIR,

letter from Baron von Zach.

" I had the honour to send to you my observations of the new planet Ceres Ferdinandea made in January, here I take the liberty to send the continuation of them made in February.

Table of observations.

1802.	Mean Time in Seeberg.	Ap. Right Ascen. observed.	App. Dec. observed.
Feb. 3	15 ^h 40' 55" S.	188° 42' 13,05"	12° 40' 5" N.
4	15 36 41,4	188 42 36,30	
5	15 32 45,1	188 42 30,15	12 50 25
9	15 16 43,7	188 38 3,90	13 14 18
19	14 34 46,7	187 58 27,90	14 20 3

Dr. Gauss has corrected his elliptical elements of the orbit upon my observations; here is what he has found since my last letter to you.

Elements,

Epoch for the beginning of the year to the meridian of Seeberg	-	-	-	-	77° 27' 36,5"
Aphelion	} both fidereal,	-	-	-	325 57 15,0
Node		-	-	-	80 58 40,0
Greatest equation of the center	-	-	-	-	9 20 8,0
Inclination of the orbit	-	-	-	-	10 37 56,6
Logarithm of $\frac{1}{2}$ axis major	-	-	-	-	0,4424742
Eccentricity of the orbit	-	-	-	-	0,0814064
Mean diurnal heliocentric and tropical motion	-	-	-	-	769",7924

With these elements of the orbit all the observations made by Mr. Piazzi in Palermo, from Jan. 1, till Feb. 11, 1801,

* The letter was sent open to Mr. Troughton for the express purpose of copying and communicating the same.

W. N.

agree

agree perfectly well, and within a few seconds; and my observations are represented by them thus:

Seeberg observ.	R. A. calculated.	Differ.	Decl. calcul.	Differ.
1801, Dec. 7	178° 33' 29,2"	— 1,4"		
1802, Jan. 11	186 45 47,6	— 2,3		
16	187 27 38,8	— 14,4		
22	188 6 18,2	— 7,6	° ' "	
25	188 20 37,2	— 2,0	11 56 58,4	+35,4"
26	188 24 37,0	— 12,5		
28	188 31 25,7	— 12,1	12 9 55,6	+14,3
29	188 34 14,1	— 4,0		
30	188 36 38,4	— 5,5	12 19 19,8	+19,1
31	188 38 38,3	— 7,1	12 24 15,3	
Feb. 3	188 42 7,8	— 5,2	12 39 53,6	— 11,4'

As these elements agree hitherto so well with the heavens, the following ephemeris calculated upon them for the next month, will probably do the same, so I annex it here to point out to the English observers the place where they have to look for the Ceres.

Position of the Ceres for Midnight Mean Time in Seeberg Observatory. Ephemeris.

1802.	R. A. in degrees.	Decl. N.	R. A. in Time.
March 1	186° 41'	15° 30'	12 ^h 26' 45"
4	186 11	15 50	12 24 45
7	185 39	16 10	12 22 36
10	185 5	16 29	12 20 18
13	184 28	16 47	12 17 53
16	183 51	17 4	12 15 24
19	183 13	17 19	12 12 50
22	182 34	17 33	12 10 15
25	181 55	17 44	12 7 40
28	181 17	17 54	12 5 7
31	189 39	18 1	12 2 37
April 3	180 3	18 6	12 0 12
6	178 29	18 10	11 57 54

This planet will come in opposition to the sun, March 17 in the afternoon. At the same time this heavenly body will be in its greatest proximity to the earth = 1,6025, and therefore

Variable light of
Ceres.

fore the most favourable time to look for its satellites, if there are any, to measure its diameter; and to examine its nebulosity. About this time, the planet will also be in its greatest geocentric latitude = $17^{\circ} 9'$, and a little later she will have her greatest retrograde motion, about 13 min. in right ascension per day. The north declination will increase till the beginning of April, and about the 9th of the same month the motion in declination will commence to the south. It appeared to me that the Ceres has some change of light; I imputed it at first to our hazy atmosphere this winter, but Mr. Schroeter of Lilienthal, and Mr. Olbers of Bremen, sent me word that they have observed the same, and they believe that it is the planet which is subject to such changes of light. Mr. Herschel will tell us best whether it is so. I have some hopes to find the planet in ancient catalogues of stars. Mr. Meßier was very near it in the year 1779. The famous comet of that year ran just over the northern wing of Virgo, as now, and the new planet was not very far distant. If the comet had attained two months sooner the completion of Virgo, Mr. Meßier must infallibly have observed the Ceres then, because he determined all the little stars in the vicinity of the comet; the planet would have been in the way of the comet, and so of course he would have caught the little planet in 1799.

If my observations are acceptable to you, dear Sir, only a little hint, and I shall continue with pleasure to give you further intelligence.

I am,
with the greatest esteem and regard,
very respectfully,
most honoured SIR,
Your obedient humble Servant,
FRANCIS BARON DE ZACH.
*Lieut. Col. and Director of Seeberg Observatory
near Gotha Saxony."*

REMARKS.

Remarks.

1. The distance which corresponds to the logarithm of $\frac{1}{4}$ axis major, viz. 0,4424742, is 2,769964, the earth's radius being unity.
2. The whole tropical period from the mean daily heliocentric tropical motion, 769,7924', is $1683^d 13^h 41' 56,5^s$.
3. The

3. The synodic revolution corresponding to this motion is $466^d 10^h 22^m$.

4. The time of opposition could not be on the 17th of March as stated in this letter, but about the 23d, as has been mentioned before, because it was on that day that the difference of the right ascensions of the sun and Ceres was 180° , even according to the Baron's own table: the error seems to have arisen from reckoning the point diametrically opposite Ceres to be nearly two degrees *short* of the equinoctial point, instead of the same quantity *over*, when the right ascension of Ceres was about 182° : the other circumstances also dependent on the moment of opposition must therefore be attributed to the 23d instead of the 17th.

5. On receiving these last corrections of Dr. Gauss I was at first surprised to find such a trifling alteration made with the greatest equation and corresponding eccentricity, after the error which I was confident I had detected; but I have now found out the cause of the apparent discrepancy, which some stress has been laid upon; the mean distance and eccentricity, I now perceive are, contrary to the usual mode of expression, given in terms of *different denominations*: the mean distance has been given in terms which suppose the radius of the *earth's orbit to be unity*, and the eccentricity is given in terms which suppose the radius of the *orbit of Ceres to be unity*, instead of its *proportional* radius 2,769964. Professor Robison on the contrary, in his approximate elements of Georgian expressed the mean distance and eccentricity in terms of the same denomination, which is also done by Lalande, Vince, and other eminent astronomers with respect to the other planets. Let us try now what the greatest equation will be by the elliptic hypothesis, when *unity* is made the radius of the orbit:

Cause of the apparent disagreement of some deductions of the author, and the elements of Mr. Gauss.

As the aphelion distance $(1 + ,08140)$ 1,08140—4,03383
Is to perihelion distance $(1 - ,08140)$ 9186—3,96313
So is tang: of $46^\circ \frac{1}{2}$ mean anom: - 10,04556

4,00869

To tang: $\frac{1}{2}$ eq. anom: $43^\circ 20' 33,6''$ - 4,03383

9,97486

Then $48^\circ 20' 33,5'' + 2 = 86^\circ 41' 7,2''$; and $92^\circ - 86^\circ 41' 7,2'' = 9^\circ 18' 5,28''$ is the greatest equation.

Also by the tabulated method we have $\frac{8140}{70500} = 08140$, which is the natural sine of $4^\circ 40' 8.27''$, or half the greatest equation $9^\circ 20' 16.51''$, which is not $9''$ above the correction of Dr. Gauss.

6. Hence it appears, that the eccentric point in the projection of the orbit of Ceres should be a little less than $\frac{1}{12}$ of the radius from the central point S (Plate XIII. Fig. 1.) which represents the sun.

W. P.

XI.

*Description of a very cheap Engine for raising Water. In a Letter from Mr. H. SARJEANT of Whitehaven, to Mr. TAYLOR, Secretary to the Society for the Encouragement of Arts.**

S I R,

Introduction.

I AM sensible that the little engine, a drawing of which accompanies this letter, can lay no great claim to novelty in its principle; nevertheless it is respectfully submitted to the consideration of the society, how far its simplicity, and cheapness of construction, may render it worthy of their attention, with a view to its being more generally known and used in similar cases.

Height of Irton-Hall 60 feet above the stream.

Irton-Hall, the seat of E. L. Irton, Esq. is situated on an ascent of sixty or sixty-one feet perpendicular height; at the foot of which, at the distance of about 140 yards from the offices, runs a small stream of water. The object was to raise this to the house for domestic purposes.

To this end a dam was made at a short distance above, so as to cause a fall of about four feet; and the water was brought by a wooden trough, into which was inserted a piece of two-inch leaden pipe, a part of which is seen at A, plate 2.

Description of the engine. A bucket is suspended at one end of a beam and a counter-weight at the other end. The stream fills the bucket and raises the counter-weight;

The stream of this pipe is so directed as to run into the bucket B, when the bucket is elevated; but so soon as it begins to descend, the stream flows over it, and goes to supply the wooden trough or well in which the foot of the forcing pump C stands, of three inches bore.

D, is an iron cylinder attached to the pump rod, which passes through it. It is filled with lead, and weighs about

* From the Transactions of the Society, for 1801, page 255. The silver medal was given to the Inventor.

240 lbs.

240 lbs. This is the power which works the pump, and forces the water through 420 feet of inch pipe from the pump up to the house.

At E is fixed a cord which, when the bucket comes to within four or five inches of its lowest projection, becomes stretched and opens a valve in the bottom of it, through which the water empties itself.

and this last in its descent works a small force-pump.

The bucket is made to empty by a valve and string.

I beg leave to add, that an engine, in a great degree similar to this, was erected some years ago by the late James Spedding, Esq. for a lead-mine near Kewick, with the addition of a smaller bucket which emptied itself into the larger, near the beginning of its descent, without which addition it was found that the beam only acquired a libratory motion, without making a full and effective stroke.

To answer this purpose in a more simple way, I constructed the small engine in such manner as to finish its stroke (speaking of the bucket end,) when the beam comes into an horizontal position, or a little below it. By this means the lever is virtually lengthened in its descent in the proportion of the radius to the cosine, of about thirty degrees, or as seven to six nearly, and consequently its power is increased in an equal proportion.

Contrivance to complete the stroke.

It is evident that the opening of the valve might have been effected, perhaps better, by a projecting pin at the bottom; but I chose to give an exact description of the engine as it stands. It has now been six months in use, and completely answers the purpose intended.

The only artists employed, except the plumber, were a country blacksmith and carpenter; and the whole cost, exclusive of the pump and pipes, did not amount to £5.

It is very cheap.

I am, Sir,

Your humble servant,

H. SARJEANT.

Warwick Court, Holborn.

Mr. CHARLES TAYLOR.

In another letter, dated Whitehaven, April 28, 1801, Mr. Sarjeant further observes that the pump requires about eighteen gallons of water in the bucket to raise the counter-weight, and make a fresh stroke in the pump; that it makes three strokes in a minute, and gives about a half-gallon into the cistern.

With a fall of 4 feet and consumption of 18 gallons, it raises half a gallon thro' 60 feet. That is to say 12 parts of tern

water raise 5 tern at each stroke. He adds, "I speak of what it did in the parts. Its rate is dryest part of last summer; when it supplied a large family, about one eighth together with work-people, &c. with water for all purposes, work; and it in a situation where none was to be had before, except some throws up 24 bad water from a common pump which has been since removed. But the above supply being more than sufficient, the machine is occasionally stopped to prevent wear, which is done by merely casting off the string of the bucket valve."

XII.

Concerning the Identity of Tellurium and Antimony, the galvanic Effects of Magnetism, and other Philosophical Subjects. By a
CORRESPONDENT.

To Mr. NICHOLSON.

S I R,

History of the
Intelligence.

ANXIOUS to learn the particulars as well as the truth of the intelligence which I received from Moravia concerning the *Tellurium* and the *decomposition of water* pretended to have been effected at Vienna by means of the magnetic fluid,* I applied to a chemical friend residing there; and it appears that this intelligence was not correct. Whence to prevent farther misinformation I conceive it my duty to hasten in publishing the following extract from my correspondent's answer to my letter, and to request you the favour of inserting it in your excellent Journal, if there be room for it.

I have the honour to be,

Sir,

Your humble servant,

N. N.

London, April 21. 1802.

Extract of a Letter from Vienna, dated 30th March, 1802.

"The intelligence communicated to you from Moravia concerning the *Tellurium* and decomposition of water was a little premature, and is not farther true than as follows:"

Account of the
experiments that
render it probable
that Tellurium
is antimony. †

"As to the first point Mr. *Tcharfky*, major in the artillery, (well known by his refutation of *Tondi's* experiments† to-

* See *Philosophical Journal*, new Series, vol. I. pag. 234.

† An account of this is given in *Baron Born's* systematic catalogue of the collection of fossils of *Mlle de Raab* at Vienna. Transl. wards

wards establishing the possibility of reducing barytes, lime and magnesia to metallic reguli,) had resolved to employ his considerable stock of *Tellurite* and *Uranite* for the purpose of subjecting these two metals to a new and closer examination. He, therefore, first prepared, according to Klaproth's manner; a regulus of Tellurium, weighing several grains. This regulus perfectly agrees with reguline antimony, *only* with respect to its *physical properties*, for instance, in the fracture, colour, hardness, specific gravity; and so likewise its oxyde perfectly resembles the *antimonium diaphoreticum ablutum* (lixivated white oxyde of antimony by nitre.) However, it would be premature, without farther experiments, in which Major Tscharky is at present engaged, directly to conclude that Tellurium is nothing else but reguline antimony."

"Major Tscharky has also prepared many ounces of the *Uranium* oxyde of Uranium, and will in a short time reduce it to a regulus of considerable size for the purpose of farther experiments."

"Concerning the second point it relates only to an experiment of Professor Jordan, who, as in the galvanic experiment for decomposing water, connected metallic wires immersed in water with the opposite poles of a magnet, and observed that an oxydation of the wires was effected. But as this experiment would not succeed in the several instances in which it was repeated, Major Tscharky and Captain Lethenyey are at present occupied in preparing a large magnetic apparatus in order to institute a regular series of experiments on this subject."

J.

Postscript. Chevalier Landriani affirms that he has discovered a method of copying old writings in the same manner, as of recently written papers, duplicates are taken by means of copying-machines.—He has also prepared an excellent indelible ink, the composition of which he will soon publish.

† The process by which Klaproth established the Tellurium as a new metal has been given in *Nicholson's Journal*, vol. II. 4to. 1799, page 273. Transl.

SCIENTIFIC

SCIENTIFIC NEWS, &c.

Notice respecting the Discovery and Situation of chromated Iron in France.

Chromate of iron.

C. Pontier has already found, three years ago, in the lower Alps, some fragments of chromated iron, out of its place; but circumstances and the war prevented him from discovering the true position of this new and curious mineral in the earth. He, however, at last found it in its natural place in a quarry near Gassin, in the road to Cavalaire.

This metal is mixed with a green serpentine rock, which probably owes its colour to the chrome, according to C. Pontier's opinion. It sometimes forms masses of five solid decimeters each.—*Bulletin des Sc. No 57.*

BOOKS OF SCIENCE,

A Treatise on Astronomy, in which the Elements of the Science are deduced in a natural order from the Appearances of the Heavens to an Observer on the Earth; demonstrated on Mathematical Principles; and explained by an Application to the various Phenomena. By OLINTHUS GREGORY, Teacher of Mathematics, Cambridge. Octavo, 522, very full Pages, with nine Copper Plates, exclusive of the Preface, Table of Contents and Index, Kearsley, London.

IT must be admitted that we have few works in our language in which the extensive science of astronomy is fully treated; and though we have several excellent popular compendiums, yet a work was wanted in which a familiar deduction of the effects from the phenomena should be combined with the stricter principles of mathematics, and in which a large quantity of useful and interesting matter should be given in the compass of an octavo volume. These are the leading advantages of the present work, besides that of its recent appearance, which naturally enabled the author to enrich his treatise with the interesting discoveries of later times.

His order consists in an investigation of the figure and dimensions of the earth; the use and meaning of the imaginary circles and points; apparent motions; causes of seasons, &c. the fixed stars; parallax, refraction, equation of time; systems of astronomy; truth of the Copernican system; theory of apparent motions; law of the planetary revolutions; real motions, rotations; phenomena of secondaries; eclipses, transits, cometary phenomena; aberration of light; elements of computation, tables, &c. &c.

L. L.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JUNE, 1802.

ARTICLE I.

On the Rev. Mr. Pearson's Analogy for deducing the greatest Equation from the Eccentricity. In a Letter from Mr. OLINTHUS GREGORY.

Cambridge, May 7, 1802.

To Mr. NICHOLSON.

SIR,

THE brilliant discoveries which have been lately made by astronomers in different parts of Europe, have naturally produced a spirit of inquiry in many persons who have hitherto paid little regard to scientific subjects, and, at the same time, have excited an earnest and active solicitude in those who have made considerable advances in the cause of science, that these inquirers be rightly directed in their pursuit. Among those who have thus laudably exerted themselves in the promotion of useful knowledge, your ingenious correspondent the Rev. Mr. Pearson, of Lincoln, (now of Parson's Green) must certainly be enumerated; and his many able communications to your Journal demand the thanks of its numerous readers. In Mr. Pearson's method of deducing the greatest-
Interesting discoveries in astronomy.
Ferdinandia, est equation.

VOL. II.—JUNE, 1802.

F

Demonstration
of its ground and
accuracy.

Ferdinandia, he mentions a simple method of deducing the greatest equation of a planet's centre from the excentricity, which he discovered a few years ago; and he invites your mathematical correspondents to give a demonstration of this method, respecting the accuracy of which he seems to have little doubt. In consequence of Mr. Pearfon's invitation, I take the liberty of addressing you on the subject. It will save much circumlocution to refer to a diagram: let, therefore, the ellipsis $APGQ$ in the annexed figure 1. Pl. VII. represent the orbit of a planet moving about the sun S in one of the foci; and let $ANQE$ be a circle described upon the major axis of the orbit as a diameter. Then, if P be the place of the planet in its orbit, AN will measure the excentric anomaly; also, if the arc AD be taken proportional to the time from the aphelion, it will represent the mean anomaly; and, letting fall the perpendicular ST from S upon NC continued, it is shewn, by *Keil* and many others, that $AN + ST = AD$. It has also been shewn that, when the equation of the centre is a maximum in any orbit, the distance SP of the planet from the sun is $= \sqrt{SA \times SQ}$, and when this is the case, in all orbits of small excentricity the point P nearly coincides with G , or the place of the planet will be near an extremity of its orbit's minor axis. In this situation of SP , it is manifest that ST will be almost coincident with SC , and nearly equal to it; SD and CN will be nearly parallel; and $DCN + SNC$ nearly equal to $2DCN$; that is, the greatest equation will be nearly equal to $2ND$, or $2ST$, or $2SC$. But, when GS the mean distance represents the radius of a circle, as the angle GCS is 90° , SC will manifestly represent the sine of an angle: therefore, as GS , the mean distance, to SC , the excentricity, so is 1, or radius, to the sine of $\frac{1}{2}$ the greatest equation, which is the same as Mr. Pearfon's analogy.

It is merely an
approximation.

It is obvious from this investigation, that the simple method pointed out by Mr. P. is merely an approximation. It supposes that the orbits are nearly circular, and will therefore produce a result deviating most widely from the truth when the excentricity is the greatest. Thus in Mr. P's table (p. 49, No. V. N. S.) the greatest error is in the orbit of mercury, where the excentricity is .20551 in terms of its own mean distance. The next greatest error is in the orbit of Mars, where the excentricity is .09308. The next error in order is that

that of Ceres, where the eccentricity is $\cdot 08140$, and error $4\frac{1}{2}''$. (Pa. 60. No. V.) In the case of the Georgian the error or difference, as stated in the table, is much too great to correspond with the eccentricity. But here I conceive there is a mistake in the determination of the eccentricity: for, according to a mean from various astronomers (*Mechain, Hennert, De la Place, Zach, and Robison,*) the proportional mean distance of the Georgian is not 1918352, but 1908352. Hence then, we have $\frac{90304}{1908352} = \cdot 0475824 = \text{nat. sine of } 2^{\circ} 43' 38\cdot 4''$, differing $\frac{4}{5}$ of a second from the determination of *La Lande*. And, in the case of Jupiter, where the eccentricity is $\cdot 04807$, the corresponding difference is $\frac{3}{100}$ of a second; which is a little larger than the former, as it ought to be. So that it appears, as well from Mr. P's table as from the investigation, that this method does not furnish a result sufficiently accurate except when the eccentricity is pretty small.

Before I conclude, I would just beg to remark, that this method, though new to Mr. Pearson, is in reality well known. The converse analogy to Mr. Pearson's long known. An analogy which is, in fact, the converse of this, though expressed rather less commodiously, is given with a demonstration, in *La Caille's Astronomy*, art. 147. *Vince's Astron.* art. 231. and my *Astron.* art. 344. And I believe it is also given in the excellent work of *La Lande*; but as I have not that performance at hand, I cannot now refer to the place. Another analogy precisely to the same effect, and deduced from the same principles, may be seen in art. 249. Book V. *Robertson's Navigation*, and under the article ECCENTRICITY in *Dr. Hutton's Math. and Phil. Dictionary*. It must afford great satisfaction to a gentleman of Mr. Pearson's candour and liberality, to find that the analogy which he accidentally struck out, is the converse of one which admits of a similar demonstration, and has been long used by astronomers as an easy and excellent approximation.

I am, Sir,

Your's with much respect,

OLINTHUS GREGORY.

II.

On the Construction of the Beams of Steam Engines. By Mr. J. C. HORNBLOWER. From the Author.

DEAR SIR,

Historical introduction.

I BEG leave through the means of your Journal, to lay before the public an account of the framed lever mentioned at the close of the article *Carpentry* in the supplement to the *Encyclopedia Britannica*, as it was originally designed for an engine to have been erected at Amsterdam in the year 1776, together with two others, possessing every possible advantage of levers consisting of small scantles.

The framed lever here described needs no hole bored in it. Other particulars.

I know not by what means the lever above referred to come to be constructed with the disadvantages intimated by the writer of that article, but there is no necessity for a hole to be bored, or a bolt to be driven in any part of the framing between the arches, except for the chain stays. The wedges *a*, *b*, Fig. 1, Plate V. thus applied, would be an improvement, for want of which a lever of this sort in the hands of a negligent engineman had one of its joggles forced off, the shoulder of the tennon, which was morticed into the arch, not being a joint by the eighth of an inch, or more; but when it met with the arch it went no further, and continued to work for many years under a great load, and much to its disadvantage in other respects.

Dimensions of a lever of indifferent workmanship.

The length of this lever was 21 feet, the scantles were 12 inches by 6; height of the whole when put together 30 inches, and leverage on the gudgeon as 4 to 3. This last circumstance operated much against its construction, by giving additional force against the joggle at that end, but had it been framed six inches higher, I doubt not but it would have stood to this day under all its disadvantages.

Scantles and load.

The sum of the scantles is 18 by 12, area of the section 216 inches, column of water in four lifts 4800 lb. with 440 fathoms of rods, (pump rods) which with the appendages on the other end, added to the power necessary to overcome the resistance, amount to about seven tons.

Another construction Fig. 2, well made.

But a much simpler, and in some respects a more advantageous mode of framing is shewn at Fig. 2, and may be constructed

fructed with or without arches. This, with little variation, is the invention of a Dutch gentleman, and was applied to the load of a 52 inch cylinder (an atmospherical engine) set up with advantages, which in point of workmanship at that time was perhaps not equalled, and therefore may be said to have been fairly tried.

This engine was calculated to raise 60,000 gallons per minute, and the scantles were 18 by 12, and 12 by 8, and where such timber can be had, it is hardly to be expected to have a lever with greater advantages than this for a single stroke, and where a double stroke is required, it may be doubled for that purpose, retaining all its principles and properties as in Fig. 3. which I suppose needs no explanation.

Dimensions and load.

Double construction.

Fig. 4. is a lever constructed by an eminent engineer in Hungary some years since, which possesses a very great degree of support by the king post and iron braces, but does not, in my opinion, discover so much science as the two preceding ones. I forgot to observe, that inner arches may be attached to Fig. 1. without materially affecting its principle, if they are well let on the whole framing, and bolted to each other without passing through the scantles.

Strong framed lever in Hungary.

I am, SIR,

Your most obedient Servant,

J. C. HORNBLOWER.

East Row, City Road, Tuesday, May 11, 1802.

III.

*On the Theory of Chemistry. In a Letter from the Rev.
J. PRIESTLEY, L. L. D. F. R. S. &c.*

To Wm. NICHOLSON, Esq.

DEAR SIR,

IN October last I sent you a reply to Mr. Cruickshank's observations on one of my arguments in support of the doctrine of phlogiston, in which I think I clearly shewed that he supposed fixed air to be formed in circumstances in which it is impossible that it should be formed, and that it is decomposed by a substance which has no such power. Having just received

Reference to former letter.
Vol. I. p. 181.

received

Mr. Cruick-
shank abandons
water as the sole
source of in-
flammable air,
which Lavoisier
thought essential
to his theory.

Inference.

ceived a letter from a friend in Paris, in which I find that great account is made of the observations of Mr. Cruickshank, so that it is now taken for granted that I must accede to the new theory, I beg you would add to my former letter, that Mr. Cruickshank himself abandons the most fundamental principle of that theory, which is, that the only source of inflammable air of any kind is water; and he makes it not necessary for that purpose. Mr. Lavoisier, treating of the inflammable air from charcoal and water, which is similar to that from charcoal and finery cinder, says, (*Elements of Chymistry*, p. 87 of the English translation) "It cannot possibly be disengaged from the charcoal, and must consequently be produced from the water." According to the new theory, the union of *oxigen*, which is supposed to come from the finery cinder, with carbon from the charcoal, must form *fixed air*, and not any kind that is inflammable. Mr. Cruickshank therefore must abandon the new theory, in order to maintain his peculiar hypothesis.

If I do not receive a better defence of this new theory from its able supporters in France, I shall conclude it to be incapable of defence, and that, as becomes ingenuous men, they will abandon it, as Mr. Cruickshank has virtually done.

I am, dear Sir,

Your's sincerely,

Northumberland, Feb. 20, 1802.

J. PRIESTLEY.

P. S. I have not any Number of your Journal of a later date than that for April last.

IV.

Experiments upon the tanning Principle; and Reflections upon the Art of Tanning. By CIT. MERAT GUILLOT, Apothecary at Auxerre.

Proust's process for obtaining tanin tedious. THE tediousness of the process indicated by Mr. Proust for obtaining the tanin, induced me to make some experiments, and to endeavour to find a more speedy method of procuring it: the following is the result of my inquiries:

1. I

1. I infused tan, in the state of fine powder, for some hours in water; I filtrated this solution, and treated it with lime water; I obtained a precipitate in considerable abundance, which I collected upon a filtre, dried, and afterwards treated with alcohol, in order to ascertain whether it were soluble in this menstruum; but the alcohol was not even coloured by it.

Infusion of tan affords a precipitate by lime water.

2. Wishing to ascertain whether the lime had a greater affinity for the acids than for the tannin with which it was combined, I treated four drachms of the precipitate of which I have spoken above, with nitric acid diluted with water, with the acid of a very gentle heat; a pretty brisk effervescence took place with a disengagement of carbonic acid gas; after four hours infusion, I filtrated the liquor, which had assumed a very deep tinge, and there remained upon the filtre a black pulverulent substance, brilliant, having an acerb and very slightly bitter taste; this residuum weighed a little less than two grammes.

Acids disengage a pulverulent matter from the lime;

3. In order to ascertain whether the nitric acid had dissolved any lime, I treated the liquid which I had filtrated with the acidulous oxalate of potash, and I obtained an abundant precipitate; on which account I conjectured, that since the nitric acid had dissolved the lime, the substance which I had obtained upon the filtre must be tannin, as the precipitate obtained by the mixture of the lime-water and of the infusion of tan, was produced by the union of the tanning principle and of the lime. In order to ascertain this point, I treated one portion of it with water, and the other with alcohol; I let these substances infuse in the sand bath for twenty-four hours:

by combining with the lime,

the water became strongly coloured, and the alcohol more so; but all the tannin (hitherto I only presume that it is such) was not dissolved; the alcohol dissolved only a little more than half of it, and the water less. I treated these two liquids, after having filtrated them, with a solution of glue, and I obtained a precipitate similar to that which is obtained by mixing infusion of tan with the same solution, but of a much darker colour, and a little less elastic. When I treated them with the muriate of tin, I obtained a precipitate which became gelatinous; when I treated them with lime-water, the tannin combined with the lime, and reproduced the tanate of lime already formed.

and the pulverulent matter is tannin.

According

According to these properties, could I doubt that this was pure tanin? certainly not, since it presents the same results as that obtained by the process of Mr. Proust.

How obtained
pure.

If we wish to obtain more pure tanin than that which is obtained after the solution of the lime of the tanate of this substance by an acid, the infusion in alcohol may be evaporated, and we shall then have very pure tanin.

The muriatic acid has presented me with the same result as the nitric.

Conjectures that
lime water is ad-
vantageous in
tanning by the
lime combining
with the tan.

The rapidity with which the upper leathers of shoes (*cuirs d'empeigne*) are tanned, according to the process of Citizen Lequin, who, in manufacturing them, contents himself with merely subjecting them to the preparations of washing and fleshing by lime water, without suffering them to swell, and afterwards tans them, led me to presume that in this case a combination is effected of tanin with the lime contained in the skin thus treated, besides the combination of the tannin with the gelatine contained in the skin, which accelerates this fabrication. May it not be probable according to this notion, that the fabrication of leather would be accelerated, if after having subjected the skins to the operations of washing and fleshing in lime water, they were left to swell in the spent ooze or water in which the old bark, which has already served for tanning leather, has been infused. In this case, the small quantity of tanin dissolved in this water would combine with the lime with which the skin would be charged in proportion to the working, and would form a tanate of lime. The swelling would perhaps be effected by this means with a little less celerity than by the sulphuric acid, but then it would perhaps be preferable, from the circumstance that the skin in swelling would begin to charge itself with tanin, whereas by the sulphuric acid, the lime with which the skin is impregnated, when worked,—this substance dissolved in water, the lime, I say, combines with the sulphuric acid employed to swell it, which, I presume, must give to the leather a brittle quality that it would perhaps not have if the other process were used. Perhaps also, after the skins have swelled in the ooze, this completion might be hastened by putting them first into the solution of tan, as Cit. Lequin does, and afterwards steeping them alternately in lime water and in infusion of tan, always taking care to leave them but a short time in the lime-water, which

which might alter them if they were left in it too long. In this case, the lime-water with which the leather would charge itself, would determine a more speedy precipitation of the tannin, and its union with the lime as well as with the gelatine contained in the leather. I believe also that by this means the leather would acquire more weight, a quality in request amongst the tanners, and perhaps it would become less permeable to water.

These are only conjectures which I advance, as they appear to me to be dictated by the theory of the art of tanning. In this instance probably, as in many others, the practice will not correspond with the theory. I recommended the trial to manufacturers, and shall make it myself when a favourable opportunity offers.

Annales de Chimie, No. 123.

V.

On the Destruction of the Grub of the Cock-chaffer. By EDWARD JONES, Esq. of Wepre-Hall, in Flintshire.*

THE grubs of the cock-chafers (or brown beetles) are white, Description of the grub of the cockchaffer. about an inch in length, and of the thickness of a turkey's quill. When disturbed they contract their length, and their bodies dilating, appear like lumps of white fat †, somewhat oval.

They inhabit sandy and light loamy soils, lie from about Situation. two to six inches deep, and may be found in spring, by paring off the sods.

This place was much infested by brown beetles; but about twelve years ago, some labourers removing a bank of earth, exposed a bed of grubs, several paces in length. Many of them were scattered among the fallen soil; and one of the men proposed to stir the surface of the bank, which being done, the grubs were seen lying in irriguous channels, as if the parent insects had dropped the eggs moving in various directions.

* Extracted from a letter to the Society for the Encouragement of Arts. Transf. for 1801.

† Hence the British name "Earth-Lard."

They are the favourite food of moles,

The same man informed me that they were the favourite food of moles; and he desired me to observe an end of the bank not stripped (being covered with mole hills); "for there no beetle grubs would be found." When opened, his remark proved true:—the moles had traced all the labyrinths of the grubs.

which on this account deserves protection.

I took the hint for the preservation of my foliage, and have ever since protected the race of moles. The brown beetles gradually decreased, and are now rarely seen here. I have not observed more than one or two stragglers in the two last springs.

General habits of moles.

Some notice of the habits of moles may be acceptable to the Society, as it has been said "that they penetrate deep into the earth in dry weather; rarely quit their subterraneous dwellings, and have few enemies;"—and "that they do great mischief in gardens and corn-grounds."

They are much on the surface while the grass is high.

I have always found that in hay and pasture grounds, as soon as the grass is high enough to cover them, they run upon the surface, where they find their food in the numerous caterpillars and insects which in the early part of the summer crawl out of the earth; and they continue above ground till the harvest. They are frequently cut by the scythe; and I have seen them at various times come out of deep hay grass into places recently mown, and, perceiving their exposure, endeavour to conceal themselves in the shorn grass.

Do not dig deep;

I have also often seen moles on very close mown grass, and bare spots in pasture land, plunge, when alarmed, among the roots; following their path (which was discernible by the heaving of the surface), I have forced them out occasionally, to try the depth of the covering, which was only a few shreds of roots.

except to avoid the plough or spade, &c.

There are two circumstances that may oblige moles sometimes to penetrate deeply:—disturbed soils in summer, such as in gardens; and ploughed light lands, where the moles delve in pursuit of worms; and, in their course, they must unroot and destroy some plants; but a vigilant gardener and husbandman will prevent much damage.

or to escape frost, &c.

The other cause of their digging deep is frost, which they avoid, or it would kill them. I have found them in winter, in peat soil, two and three feet below the surface; and in the hard frost of 1794-5 (cutting deep trenches to separate grounds,)

grounds), I found moles several mornings, that had worked through and fallen into the trenches, frozen to death.

Their summer emerſion is proved by the birds of prey: Birds of prey deſtroy great numbers while above ground. they deſtroy great numbers of moles. This year there were taken out of one kite's neſt twenty-two moles, and out of another fifteen, ſome of which were putrid; beſides many frogs and unfledged birds.

The rapacity of the kites ſhews that they are deſtructive enemies to the moles, which, if moles are ſerviceable to man, ſhould be known, that he may ſtay his arm.

Moles are frequently found dead upon the graſs in ſummer, with marks of having been bitten, as if to ſuck their blood, but with no part of their bodies conſumed. This, I ſuppoſe, is done by weaſels; and the following (not very common) occurrence, which happened in the ſummer of 1739, tends to prove it:—

A kite was obſerved riſing from the ground with ſome prey, and inſtead of flying to an adjoining wood, he ſoared almoſt perpendicularly. After remaining a ſhort time ſtationary, he came gradually down, with his wings extended and motionleſs, and dropt very near the place from which he had riſen *. Incident of a kite deſtroyed by a weaſel, &c.

Several perſons who were near, and ſaw the flight and deſcent, ran immediately to the ſpot, and a weaſel darted from the kite, which they found dead; and they diſcovered, on examination, that the kite had been bit in the throat, and bled to death. Near it they found a dead mole, yet warm, which was bitten in the neck; and they concluded that the weaſel had cauſed the death of both.

In ſeveral parts of the kingdom where I have met with a great number of brown beetles, moles were regularly deſtroyed; and in Staffordſhire, being ſhown ſeveral large trees covered by beetles, and totally deſolated, I enquired whether they deſtroyed the moles? The anſwer was, that they did, or they ſhould be over-run with them. Brown beetles are abundant where the moles are deſtroyed.

The loſs of foliage not being of great conſequence to the farmer, he is ſatisfied that his turkeys make him amends in other reſpects, by eating the brown beetles, of which they are very fond, and which they ſwallow voraciouſly. Other remarks,

* A ſimilar circumſtance was mentioned in the Cheſter papers, three or four years ago.

A gen-

A gentleman informed me lately, that rooks also eat the beetles.

But these means are confined to the winged beetle. It appears to me that the mole is the only certain destroyer of the grub.

My hay and pasture grounds are, every spring, thickly studded with mole hillocks. They are scattered in the usual manner; and when the grasses are up, the moles cease to work, and scarce a hillock appears till after harvest.

VI.

Methods of diminishing the Irregularities of Time-Pieces, arising from differences in the Arc of Vibration of the Pendulum. By Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

S I R,

Lynn Regis, May 17, 1802,

Chronometers,
&c. rendered ir-
regular by the
oil.

AFTER all the improvements which the mechanism of clocks and watches has received, there are still obstacles which stand in the way of an exact performance. The use of oil in chronometers has long been complained of, but still remains a necessary evil; and that variation which obtains in the arc of vibration of the pendulum, is a source of much error in clocks.

Transit clock at
Greenwich varies
in its arc of
vibration.

The transit clock at Greenwich sometimes varies in its semi-arc of vibration *twenty* minutes in a year, and a compound pendulum which came under my own inspection some years ago, varied in its arc nearly as much.

Old remedy for
the inequality of
the arcs.

Many years have elapsed since it was discovered, that the short vibrations were performed in less time than the long ones, and methods were used to remove the inconvenience. HUYGENS proposed a method by which the centre of oscillation might be made to vibrate in the arc of a cycloid, and demonstrated that a pendulum moving in that curve, would perform all its vibrations, whether long or short, in equal times: and others proposed to remove the evil by a peculiar form of the pallets.

These

These contrivances, how ingenious soever they may appear in theory, have not answered any good purpose in practice; but, what may be impracticable to perform by one method, may not be found so by another.

The spring by which the pendulum is suspended seems capable of receiving such improvement, as would render the long and short vibrations isochronal, without any other alteration in the mechanism of the clock. This improvement consists in cutting the spring broader at the top than at the bottom, that it may not bend totally at the point of suspension. The pendulum would, in that case, when in motion, become shorter at the ends of the arc, consequently it would no longer vibrate in the arc of a circle, and on this account there is some reason for supposing that the long vibrations would be performed in less time than they were before.

When this method is put in practice, the spring should be made so broad at the point of suspension at first, that the long vibrations may be performed in less time than the short ones, and then made narrower till all the vibrations be performed in equal times. But I supposed that this method would be attended with much trouble, and that it would be better to remove the cause than to counteract its effects.

When I made observations on the pendulum above-mentioned, I found that it was much influenced by the weather. In cold and moist weather it moved through a larger arc, than when the atmosphere was dry and warm.

As all metals alter their dimensions, and consequently their elastic force by heat and cold, it follows that that part of the clock called the *crutch*, would act more forcibly on the pendulum in cold weather than in warm; and it seems probable, that this variation would be greater in a weak *crutch* than in a strong one.

About seven years ago having a regulator made by Mr. James Bullock, of Holborn, and being desirous of putting my theory to the test of experience, I had the *crutch* made exceedingly strong, and firmly connected with the pallets, that the maintaining power of the clock might be communicated to the pendulum uniformly in all states of the atmosphere. The clock case is made very thick of solid mahogany, and upon the *rising board**, which is two inches thick, stands an an-

* The board on which the wheel-work stands.

gular piece of brass (thus A) to support the pendulum. This support is made fast with screws to the *rising-board* at the bottom, and to the *frame-plate* of the clock at the top. Who invented this support I know not, but it appears a very excellent method of giving stability to the point of suspension of the pendulum.

Refect.

This clock has now been going near seven years. It vibrates $1^{\circ} 49'$ on each side of the perpendicular, from which I have not seen it vary more than $2'$, except once in a very hard frost, and as it seldom varies so much as $2'$ in its semi-arc, this cause of error in the pendulum seems to be very nearly removed. It has been cleaned only once since it came into my possession, but this made no alteration, either in its arc of vibration or its rate of going.

I am, with much respect,

Sir,

Your very humble servant,

EZEKIEL WALKER.

VII.

On the Theory of Light and Colours *. By THOMAS YOUNG, M. D. F. R. S. Professor of Natural Philosophy in the Royal Institution.

Importance of general principles in science.

ALTHOUGH the invention of plausible hypotheses, independent of any connection with experimental observations, can be of very little use in the promotion of natural knowledge; yet the discovery of simple and uniform principles, by which a great number of apparently heterogeneous phenomena are reduced to coherent and universal laws, must ever be allowed to be of considerable importance towards the improvement of the human intellect.

Object of the present dissertation.

The object of the present dissertation is not so much to propose any opinions which are absolutely new, as to refer some theories, which have been already advanced, to their original inventors, to support them by additional evidence, and to apply them to a great number of diversified facts, which have hitherto been buried in obscurity. Nor is it absolutely ne-

* In support of the truth of that hypothesis, which ascribes the phenomena to undulatory motions of an extremely elastic and rare fluid. From the Philof. Trans. 1802.—N.

cessary in this instance to produce a single new experiment; for of experiments there is already an ample store, which are so much the more unexceptionable, as they must have been conducted without the least partiality for the system by which they will be explained; yet some facts, hitherto unobserved, will be brought forwards, in order to shew the perfect agreement of that system with the multifarious phenomena of nature.

The optical observations of Newton are yet unrivalled; and, excepting some casual inaccuracies, they only rise in our estimation, as we compare them with later attempts to improve on them. A further consideration of the colours of thin plates, as they are described in the second book of Newton's optics, has converted that prepossession which I before entertained for the undulatory system of light, into a very strong conviction of its truth and sufficiency; a conviction which has been since most strikingly confirmed, by an analysis of the colours of striated substances. The phenomena of thin plates are indeed so singular, that their general complexion is not without great difficulty reconcilable to any theory, however complicated, that has hitherto been applied to them; and some of the principal circumstances have never been explained by the most gratuitous assumptions; but it will appear, that the minutest particulars of these phenomena are not only perfectly consistent with the theory which will now be detailed, but that they are all the necessary consequences of that theory, without any auxiliary suppositions; and this by inferences so simple, that they become particular corollaries, which scarcely require a distinct enumeration.

Excellence of the optical observations of Newton.

Phenomena of thin plates.

A more extensive examination of Newton's various writings has shown me, that he was in reality the first that suggested such a theory as I shall endeavour to maintain; that his own opinions varied less from this theory than is now almost universally supposed; and that a variety of arguments have been advanced, as if to confute him, which may be found nearly in a similar form in his own works; and this by no less a mathematician than Leonard Euler, whose system of light, as far as it is worthy of notice, either was, or might have been, wholly borrowed from Newton, Hooke, Huygens, and Malebranche.

Newton first suggested the theory of undulation.

Euler.

Those who are attached, as they may be with the greatest justice, to every doctrine which is stamped with the Newtonian approbation, will probably be disposed to bestow on these considerations

Reference to the works of Newton.

considerations so much the more of their attention, as they appear to coincide more nearly with Newton's own opinions. For this reason, after having briefly stated each particular position of my theory, I shall collect, from Newton's various writings, such passages as seem to be the most favourable to its admission; and, although I shall quote some papers which may be thought to have been partly retracted at the publication of the optics, yet I shall borrow nothing from them that can be supposed to militate against his maturer judgment.

HYPOTHESIS I.

A luminiferous Ether pervades the Universe, rare and elastic in a high degree.

Passages from Newton.

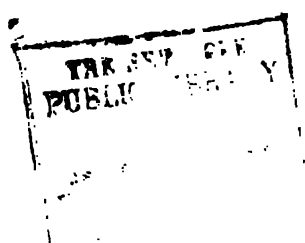
Hypoth. I.
Universality of
the ether.

"The hypothesis certainly has a much greater affinity with his own," that is, Dr. Hooke's, "hypothesis, than he seems to be aware of; the vibrations of the ether being as useful and necessary in this as in his." (Phil. Transf. Vol. VII. p. 5037. Abr. Vol. I. p. 145. Nov. 1672.)

"To proceed to the hypothesis: first, it is to be supposed therein, that there is an ethereal medium, much of the same constitution with air, but far rarer, subtler, and more strongly elastic. It is not to be supposed, that this medium is one uniform matter, but compounded, partly of the main phlegmatic body of ether, partly of other various ethereal spirits, much after the manner that air is compounded of the phlegmatic body of air, intermixed with various vapours and exhalations: for the electric and magnetic effluvia, and gravitating principle, seem to argue such variety." (Birch. Hist. of R. S. Vol. III. p. 249. Dec. 1675.)

Arguments pro-
posed by New-
ton in support of
the ether.

"Is not the heat (of the warm room) conveyed through the vacuum by the vibrations of a much subtler medium than air? And is not this medium the same with that medium by which light is refracted and reflected, and by whose vibrations light communicates heat to bodies, and is put into fits of easy reflection, and easy transmission? And do not the vibrations of this medium in hot bodies, contribute to the intenseness and duration of their heat? And do not hot bodies communicate their heat to contiguous cold ones, by the vibrations of this medium propagated from them into the cold ones?"



W. Russell's improvements in the hydraulic Engines.

Fig. 3. Pressure Engine.

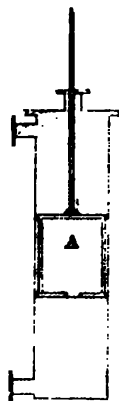


Fig. 1. at Schemnitz.

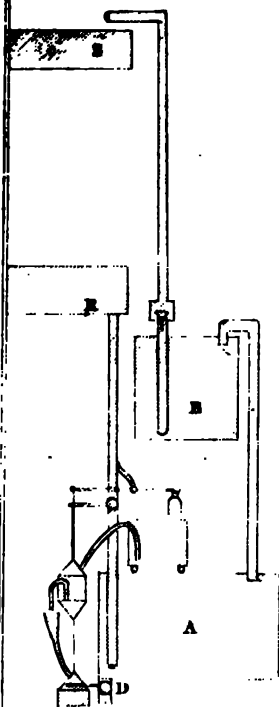
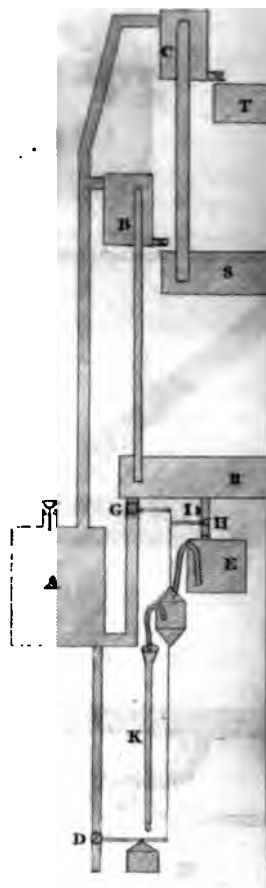

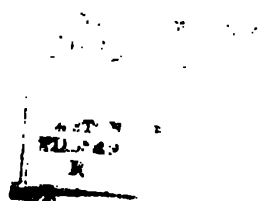


Fig. 2. M. Goodwyn's.





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TILDEN FOUNDATIONS

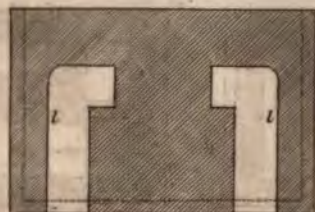


Stove of Cit. Guyton.

Fig. 1.



Fig. 2.



Section of the Stove of Guxton.

Fig. 7.



Fig. 4.

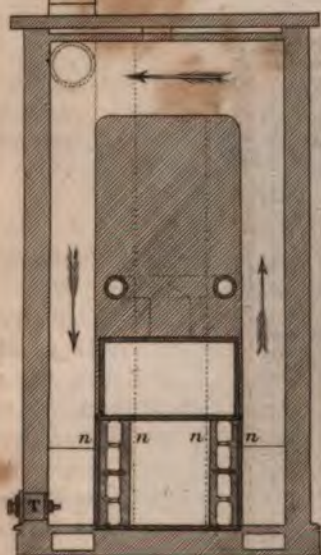


Fig. 6.

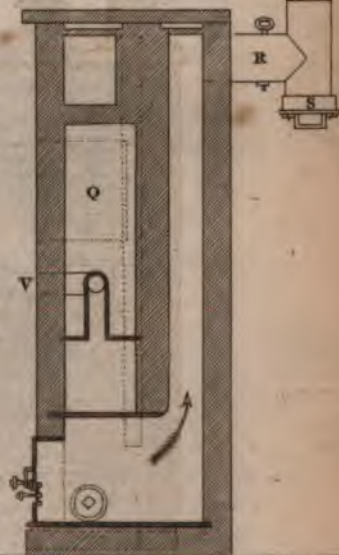


Fig. 3.

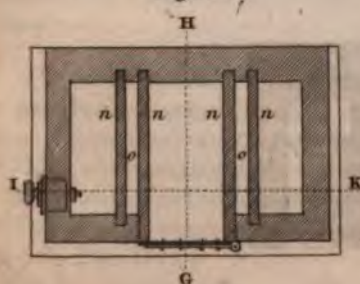


Fig. 5.



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TILDEN FOUNDATION

" ones? And is not this medium exceedingly more rare and
 " subtile than the air, and exceedingly more elastic and active?
 " And doth it not readily pervade all bodies? And is it not,
 " by its elastic force, expanded through all the heavens?—
 " May not planets and comets, and all gross bodies, perform
 " their motions in this ethereal medium? And may not its re-
 " sistance be so small, as to be inconsiderable? For instance, if
 " this ether (for so I will call it) should be supposed 700,000
 " times more elastic than our air, and above 700,000 times
 " more rare, its resistance would be about 600,000,000 less
 " than that of water. And so small a resistance would scarce
 " make any sensible alteration in the motions of the planets, in
 " ten thousand years. If any one would ask how a medium
 " can be so rare, let him tell me, How an electric body can by
 " friction emit an exhalation so rare and subtile, and yet so
 " potent? And how the effluvia of a magnet can pass through
 " a plate of glass without resistance, and yet turn a magnetic
 " needle beyond the glass?" (Optics, Qu. 18, 22.)

HYPOTHESIS II.

Undulations are excited in this Ether whenever a body becomes luminous.

Scholium. I use the word undulation, in preference to vi-
 bration, because vibration is generally understood as implying a
 motion which is continued alternately backwards and forwards,
 by a combination of the momentum of the body with an acce-
 lerating force, and which is naturally more or less permanent;
 but an undulation is supposed to consist in a vibratory motion,
 transmitted successively through different parts of a medium,
 without any tendency in each particle to continue its motion,
 except in consequence of the transmission of succeeding undu-
 lations, from a distinct vibrating body; as, in the air, the vi-
 brations of a chord produce the undulations constituting
 sound.

Hypoth. II.
Light consists
in ethereal un-
dulation.

Passages from Newton.

" Were I to assume a hypothesis, it should be this, if pro-
 " pounded more generally, so as not to determine what light
 " is, further than that it is something or other capable of ex-
 " citing vibrations in the ether; for thus it will become so ge-
 " neral and comprehensive of other hypotheses, as to leave
 " little room for new ones to be invented." (Birch. Vol. III.
 p. 249. Dec. 1675.)

Detail of the
hypoth. by
Newton. Light.

THEORY OF LIGHT AND COLOURS.

"In the second place, it is to be supposed, that the ether is
 "a vibrating medium like air, only the vibrations far more
 "swift and minute; those of air, made by a man's ordinary
 "voice, succeeding one another at more than half a foot, or a
 "foot distance; but those of ether at a less distance than the
 "hundred thousandth part of an inch. And, as in air the vi-
 "brations are some larger than others, but yet all equally swift,
 "(for in a ring of bells the sound of every tone is heard at two
 "or three miles distance, in the same order that the bells are
 "struck), so, I suppose, the ethereal vibrations differ in big-
 "ness, but not in swiftness. Now, these vibrations, beside
 "their use in reflection and refraction, may be supposed the
 "chief means by which the parts of fermenting or putrifying
 "substances, fluid liquors, or melted, burning, or other hot
 "bodies, continue in motion." (Birch. Vol. III. p. 231.
 Dec. 1675).

reflection,
 tion;

"When a ray of light falls upon the surface of any pellucid
 "body, and is there refracted or reflected, may not waves of
 "vibrations, or tremors, be thereby excited in the refracting
 "or reflecting medium? And are not these vibrations propa-
 "gated from the point of incidence to great distances? And
 "do they not overtake the rays of light, and by overtaking
 "them successively, do not they put them into the fits of easy
 "reflection and easy transmission described above?" (Optics,
 Qu. 17).

and the alter-
 nate fits.

"Light is in fits of easy reflection and easy transmission, be-
 "fore its incidence on transparent bodies. And probably it is
 "put into such fits at its first emission from luminous bodies,
 "and continues in them during all its progress." (Optics,
 Second Book, Part III. Prop. 13.)

HYPOTHESIS III.

*The Sensation of different Colours depends on the different frequency
 of Vibrations, excited by Light in the Retina.*

Passages from Newton.

Hypothesis III.
 Colour depends
 on the frequency
 of the ethereal
 vibrations.

"The objector's hypothesis, as to the fundamental part of
 it, is not against me. That fundamental supposition is, that
 the parts of bodies, when briskly agitated, do excite vibrations
 in the ether, which are propagated every way from those bo-
 dies in straight lines, and cause a sensation of light by beating
 and

and dashing against the bottom of the eye, something after the manner that vibrations in the air cause a sensation of sound by beating against the organs of hearing. Now, the most free and natural application of this hypothesis to the solution of phenomena, I take to be this: that the agitated parts of bodies, according to their several sizes, figures, and motions, do excite vibrations in the ether of various depths or bignesses, ^{Particular development;} which, being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white colour; but if by any means those of unequal bignesses be separated from one another, the largest beget a sensation of a red colour, the least or shortest of a deep violet, and the intermediate ones of intermediate colours; much after the manner that bodies, ^{as in sound.} according to their several sizes, shapes, and motions, excite vibrations in the air of various bignesses, which, according to those bignesses, make several tones in sound: that the largest vibrations are best able to overcome the resistance of a refracting superficies, and so break through it with least refraction; ^{Various refrangibility:} whence the vibrations of several bignesses, that is, the rays of several colours, which are blended together in light, must be parted from one another by refraction, and so cause the phenomena of prisms, and other refracting substances; and that it depends on the thickness of a thin transparent plate or bubble, ^{Thin transparent plates.} whether a vibration shall be reflected at its further superficies, or transmitted; so that, according to the number of vibrations, interceding the two superficies, they may be reflected or transmitted for many successive thicknesses. And, since the vibrations which make blue and violet, are supposed shorter than those which make red and yellow, they must be reflected at a less thickness of the plate: which is sufficient to explicate all the ordinary phenomena of those plates or bubbles, and also of all natural bodies, whose parts are like so many fragments of such plates. These seem to be the most plain, genuine, and necessary conditions of this hypothesis. And they agree so justly with my theory, that if the animadversor think fit to apply them, he need not, on that account, apprehend a divorce from it. But yet, how he will defend it from other difficulties, I know not." (Phil. Transf. Vol. VII. p. 5088. Abr. Vol. I. p. 145. Nov. 1672.)

"To explain colours, I suppose, that as bodies of various sizes, densities, or sensations, do by percussion or other action ^{Repetition of the theory.}

excite sounds of various tones, and consequently vibrations in the air of different bigness; so the rays of light, by impinging on the stiff refracting superficies, excite vibrations in the ether, of various bigness; the biggest, strongest, or most potent rays, the largest vibrations; and others shorter, according to their bigness, strength, or power: and therefore the ends of the capillamenta of the optic nerve, which pave or face the retina, being such refracting superficies, when the rays impinge upon them, they must there excite these vibrations, which vibrations (like those of sound in a trunk or trumpet) will run along the aqueous pores or crystalline pith of the capillamenta, through the optic nerves, into the sensorium; and there, I suppose, affect the sense with various colours, according to their bigness and mixture; the biggest with the strongest colours, reds and yellows; the least with the weakest, blues and violets; the middle with green; and a confusion of all with white, much after the manner that, in the sense of hearing, nature makes use of aerial vibrations of several bignesses, to generate sounds of divers tones; for the analogy of nature is to be observed." (Birch, Vol. III. p. 262, Dec. 1675.)

"Considering the lastingness of the motions excited in the bottom of the eye by light, are they not of a vibrating nature? Do not the most refrangible rays excite the shortest vibrations, —the least refrangible the largest? May not the harmony and discord of colours arise from the proportions of the vibrations propagated through the fibres of the optic nerve into the brain, as the harmony and discord of sounds arise from the proportions of the vibrations of the air?" (Optics, Qu. 16, 13, 14.)

Scholium. The parts of the retina being probably capable of vibrating in unison with a limited number of colorific motions,

Scholium. Since, for the reason here assigned by Newton, it is probable that the motion of the retina is rather of a vibratory than of an undulatory nature, the frequency of the vibrations must be dependent on the constitution of this substance. Now, as it is almost impossible to conceive each sensitive point of the retina to contain an infinite number of particles, each capable of vibrating in perfect unison with every possible undulation, it becomes necessary to suppose the number limited, for instance, to the three principal colours, red, yellow, and blue, of which the undulations are related in magnitude nearly as the numbers 8, 7, and 6; and that each of the particles is capable of being put in motion less or more forcibly, by undulations

tions differing less or more from a perfect unison; for instance, the undulations of green light being nearly in the ratio of $6\frac{1}{2}$, will affect equally the particles in unison with yellow and blue, and produce the same effect as a light composed of those two species: and each sensitive filament of the nerve may consist of three portions, one for each principal colour. Allowing this it is not to be statement, it appears that any attempt to produce a musical effect from colours, must be unsuccessful, or at least that no- colours can have a musical effect. thing more than a very simple melody could be imitated by them; for the period, which in fact constitutes the harmony of any concord, being a multiple of the periods of the single undulations, would in this case be wholly without the limits of sympathy of the retina, and would lose its effect; in the same manner as the harmony of a third or a fourth is destroyed, by depressing it to the lowest notes of the audible scale. In hearing, there seems to be no permanent vibration of any part of the organ. The ear not permanently affected.

HYPOTHESIS IV.

All material Bodies have an attraction for the ethereal Medium, by means of which it is accumulated within their substance, and for a small Distance around them, in a State of greater Density, but not of greater Elasticity. Hypoth. IV. The density but not the elasticity of the ether, is greater within and near other bodies.

It has been shewn, that the three former hypotheses, which may be called essential, are literally parts of the more complicated Newtonian system. This fourth hypothesis differs perhaps in some degree from any that have been proposed by former authors, and is diametrically opposite to that of Newton; but, both being in themselves equally probable, the opposition is merely accidental; and it is only to be inquired which is the best capable of explaining the phenomena. Other suppositions might perhaps be substituted for this, and therefore I do not consider it as fundamental, yet it appears to be the simplest and best of any that have occurred to me. Newton supposed the contrary.

PROPOSITION I.

All impulses are propagated in a homogeneous elastic Medium with an equable velocity. Propos. I. Impulse is propagated uniformly in an homogeneous elastic medium.

Every experiment relative to sound coincides with the observation already quoted from Newton, that all undulations are propagated through the air with equal velocity; and this is further

ther confirmed by calculations. (Lagrange, Misc. Taur. Vol. I. p. 91. Also, much more concisely, in my Syllabus of a course of Lectures on Natural and Experimental Philosophy, about to be published. Article 289.) If the impulse be so great as materially to disturb the density of the medium, it will be no longer homogeneous; but, as far as concerns our senses, the quantity of motion may be considered as infinitely small. It is surprising that Euler, although aware of the matter of fact, should still have maintained, that the more frequent undulations are more rapidly propagated. (Theor. mus. and Conject. phys.) It is possible, that the actual velocity of the particles of luminiferous ether may bear a much less proportion to the velocity of the undulations than in sound; for light may be excited by the motion of a body moving at the rate of only one mile in the time that light moves an hundred millions.

Law of the velocities in different mediums.

Scholium 1. It has been demonstrated, that in different mediums the velocity varies in the subduplicate ratio of the force directly, and of the density inversely. (Misc. Taur. Vol. I. p. 91. Young's Syllabus. Art. 294.)

Undulations do not mix.

Scholium 2. It is obvious, from the phenomena of elastic bodies and of sounds, that the undulations may cross each other without interruption. But there is no necessity that the various colours of white light should intermix their undulations; for, supposing the vibrations of the retina to continue but a five hundredth of a second after their excitement, a million undulations of each of a million colours may arrive in distinct succession within this interval of time, and produce the same sensible effect, as if all the colours arrived precisely at the same instant.

PROPOSITION II.

Prop. II. Nature of undulation.

An Undulation conceived to originate from the Vibration of a single Particle, must expand through a homogeneous Medium in a spherical Form, but with different Quantities of Motion in different Parts.

For, since every impulse, considered as positive or negative, is propagated with a constant velocity, each part of the undulation must in equal times have past through equal distances from the vibrating point. And, supposing the vibrating particle, in the course of its motion, to proceed forwards to a small distance in a given direction, the principal strength of the undulation

ation will naturally be straight before it; behind it, the motion will be equal, in a contrary direction; and, at right angles to the line of vibration, the undulation will be evanescent.

Now, in order that such an undulation may continue its progress to any considerable distance, there must be in each part of it, a tendency to preserve its own motion in a right line from the centre; for, if the excess of force at any part were communicated to the neighbouring particles, there can be no reason why it should not very soon be equalised throughout, or, in other words, become wholly extinct, since the motions in contrary directions would naturally destroy each other. The origin of sound from the vibration of a chord is evidently of this nature; on the contrary, in a circular wave of water, every part is at the same instant either elevated or depressed. It may be difficult to show mathematically, the mode in which this inequality of force is preserved; but the inference from the matter of fact, appears to be unavoidable; and, while the science of hydrodynamics is so imperfect that we cannot even solve the simple problem of the time required to empty a vessel by a given aperture, it cannot be expected that we should be able to account perfectly for so complicated a series of phenomena, as those of elastic fluids. The theory of Huygens indeed explains the circumstance in a manner tolerably satisfactory: he supposes every particle of the medium to propagate a distinct undulation in all directions; and that the general effect is only perceptible where a portion of each undulation conspires in direction at the same instant; and it is easy to show that such a general undulation would in all cases proceed rectilinearly, with proportionate force; but, upon this supposition, it seems to follow, that a greater quantity of force must be lost by the divergence of the partial undulations, than appears to be consistent with the propagation of the effect to any considerable distance. Yet it is obvious, that some such limitation of the motion must naturally be expected to take place; for, if the intensity of the motion of any particular part, instead of continuing to be propagated straight forwards, were supposed to affect the intensity of a neighbouring part of the undulation, an impulse must then have travelled from an internal to an external circle in an oblique direction, in the same time as in the direction of the radius, and consequently with a greater velocity; against the first proposition.

proposition. In the case of water, the velocity is by no means so rigidly limited as in that of an elastic medium. Yet it is not necessary to suppose, nor is it indeed probable, that there is absolutely not the least lateral communication of the force of the undulation, but that, in highly elastic mediums, this communication is almost insensible. In the air, if a chord be perfectly insulated, so as to propagate exactly such vibrations as have been described, they will in fact be much less forcible than if the chord be placed in the neighbourhood of a sounding board, and probably in some measure because of this lateral communication of motions of an opposite tendency. And the different intensity of different parts of the same circular undulation may be observed, by holding a common tuning fork at arm's length, while sounding, and turning it, from a plane directed to the ear, into a position perpendicular to that plane.

PROPOSITION III.

Proposition III.
Lateral undulations explained.

A Portion of a spherical Undulation, admitted through an Aperture into a quiescent Medium, will proceed to be further propagated rectilinearly in concentric Superficies, terminated laterally by weak and irregular Portions of newly diverging Undulations.

At the instant of admission, the circumference of each of the undulations may be supposed to generate a partial undulation, filling up the nascent angle between the radii and the surface terminating the medium; but no sensible addition will be made to its strength by a divergence of motion from any other parts of the undulation, for want of a coincidence in time, as has already been explained with respect to the various force of a spherical undulation. If indeed the aperture bear but a small proportion to the breadth of an undulation, the newly generated undulation may nearly absorb the whole force of the portion admitted; and this is the case considered by Newton in the Principia. But no experiment can be made under these circumstances with light, on account of the minuteness of its undulations, and the interference of inflection; and yet some faint radiations do actually diverge beyond any probable limits of inflection, rendering the margin of the aperture distinctly visible in all directions; these are attributed by Newton to some unknown cause, distinct from inflection: (Optics,

Third

Third Book, Obf. 5. and they fully answer the description of this propofition.

Let the concentric lines in Fig. 1. (Plate V.) represent the contemporaneous fition of fimilar parts of a number of fucceffive undulations diverging from the point A; they will alfo represent the fucceffive fitions of each individual undulation: let the force of each undulation be represented by the breadth of the line, and let the cone of light ABC be admitted through the aperture BC; then the principal undulations will proceed in a rectilinear direction towards GH, and the faint radiations on each fide will diverge from B and C as centres, without receiving any additional force from any intermediate point D of the undulation, on account of the inequality of the lines DE and DF. But, if we allow fome little lateral divergence from the extremities of the undulations, it muft diminifh their force, without adding materially to that of the diffipated light; and their termination, inftead of the right line BG, will affume the form CH; fince the lofs of force muft be more confiderable near to C than at greater diftances. This line corresponds with the boundary of the fhadow in Newton's firft obfervation, Fig. 1; and it is much more probable that fuch a diffipation of light was the caufe of the increafe of the fhadow in that obfervation, than that it was owing to the action of the infecting atmofphere, which muft have extended a thirtieth of an inch each way in order to produce it; efpecially when it is confidered that the fhadow was not diminifhed by furrounding the air with a denfer medium than air, which muft in all probability have weakened and contracted its infecting atmofphere. In other circumftances, the lateral divergence might appear to increafe, inftead of diminifhing, the breadth of the beam.

As the fubject of this propofition has always been efteemed the moft difficult part of the undulatory fystem, it will be proper to examine here the objections which Newton has grounded upon it.

Objections of
Newton to the
undulatory fy-
ftem of light.

“ To me, the fundamental fuppoftition itfelf feems impoffible; namely, that the waves or vibrations of any fluid can, like the rays of light, be propagated in ftraight lines, without a continual and very extravagant fpreading and bending every way into the quiefcent medium, where they are terminated by it.

it. I mistake, if there be not both experiment and demonstration to the contrary." (Phil. Transf. VII. 5089, Abr. I. 146. Nov. 1672.)

"*Motus omnis per fluidum propagatus divergit a recto tramite in spatia immota.*"

"*Quoniam medium ibi,*" in the middle of an undulation admitted, "*densius est, quam in spatiis hinc inde, dilatabit sese tam versus spatia utrinque fila, quam versus pulsuum rurius intervalle; eoque pacto—pulsus eadem fere celeritate sese in medii partes quiescentes hinc inde relaxare debent;—ideoque spatium totum occupabunt.—Hoc experimur in sonis.*" (Princip. Lib. II. Prop. 42.)

"Are not all hypotheses erroneous, in which light is supposed to consist in pressure or motion, propagated through a fluid medium?—If it consisted in pressure or motion, propagated either in an instant, or in time, it would bend into the shadow. For pressure or motion cannot be propagated in a fluid in right lines beyond an obstacle which stops part of the motion, but will bend and spread every way into the quiescent medium which lies beyond the obstacle. The waves on the surface of stagnating water, passing by the sides of a broad obstacle which stops part of them, bend afterwards, and dilate themselves gradually into the quiet water behind the obstacle. The waves, pulses, or vibrations of the air, wherein sounds consist, bend manifestly, though not so much as the waves of water. For a bell or a cannon may be heard beyond a hill, which intercepts the sight of the sounding body; and sounds are propagated as readily through crooked pipes as straight ones. But light is never known to follow crooked passages, nor to bend into the shadow. For the fixed stars, by the interposition of any of the planets, cease to be seen, And so do the parts of the sun, by the interposition of the moon, Mercury, or Venus. The rays which pass very near to the edges of any body, are bent a little by the action of the body; but this bending is not towards but from the shadow, and is performed only in the passage of the ray by the body, and at a very small distance from it. So soon as the ray is past the body, it goes right on." (Optics, Qu. 28.)

Answers to the
objections of
Newton.

Now the proposition quoted from the Principia does not directly contradict this proposition; for it does not assert that such a motion must diverge equally in all directions; neither
can

can it with truth be maintained, that the parts of an elastic medium communicating any motion, must propagate that motion equally in all directions. (Phil. Transf. for 1800. p. 109—112.) All that can be inferred by reasoning is, that the marginal parts of the undulation must be somewhat weakened, and that there must be a faint divergence in every direction; but whether either of these effects might be of sufficient magnitude to be sensible, could not have been inferred from argument, if the affirmative had not been rendered probable by experiment.

As to the analogy with other fluids, the most natural inference from it is this: "The waves of the air, wherein sounds consist, bend manifestly, though not so much as the waves of water;" water being an inelastic, and air a moderately elastic medium; but ether being most highly elastic, its waves bend very far less than those of the air, and therefore almost imperceptibly. Sounds are propagated through crooked passages, because their sides are capable of reflecting sound, just as light would be propagated through a bent tube, if perfectly polished within.

Sound deflects less than waves of water.

Crooked passages.

The light of a star is by far too weak to produce, by its faint divergence, any visible illumination of the margin of a planet eclipsing it; and the interception of the sun's light by the moon, is as foreign to the question, as the statement of inflection is inaccurate.

To the argument adduced by Huygens, in favour of the rectilinear propagation of undulations, Newton has made no reply; perhaps because of his own misconception of the nature of the motions of elastic mediums, as dependent on a peculiar law of vibration, which has been corrected by later mathematicians. (Phil. Transf. for 1800, p. 116.) On the whole, it is presumed, that this proposition may be safely admitted, as perfectly consistent with analogy and with experiment.

(To be continued.)

Page 91

VIII.

*Remarks on Combustion. By THOMAS THOMSON, M. D.
Lecturer on Chemistry in Edinburgh.*

(Concluded from Page 20.)

Nitric acid
formed sponta-
neously ;

and also by elec-
tricity, through
common air, and
probably by gal-
vanism :

Apparently by
decomposition.

Nitric acid in
the process for
composing wa-
ter.

VI. NITRIC acid is formed spontaneously upon the surface of the earth by processes with which we are but imperfectly acquainted ; but which certainly have no resemblance to combustion. Its oxygen is probably furnished by the *air*, which is a supporter ; at least, it has been observed, that if azote, the instant it is evolved, comes in contact with air, it is capable of combining with its oxygen, and forming nitric acid.

Nitric acid may be formed also, as Mr. Cavendish has demonstrated, by passing electric sparks through common air, a supporter. In all probability it may be formed also by the galvanic pile, but this may be considered as equivalent to electricity. This formation of nitric acid by means of electricity, has been considered as a combustion, but for what reason it is not easy to say : the substance acted upon is not a combustible with a supporter, but a supporter alone. Electricity is so far from being equivalent to combustion, that it sometimes acts in a manner diametrically opposite ; *unburning*, if I may use the expression, a substance which has already undergone combustion, and converting a *product* into a *combustible* and a *supporter*. Thus it decomposes water, and converts it into oxygen and hydrogen gas ; therefore it must be capable of supplying the substances which the oxygen and combustible lose when they combine by combustion, and form a product*.

There is one process more, during which nitric acid is formed, which must at first sight appear an exception to the general rule ; I mean the formation of nitric acid, which takes place during the combustion of hydrogen gas in oxygen gas contaminated with air. But in this case it is the *hydrogen* only which burns, and not the *air* ; the air indeed combines intimately, and forms nitric acid, just as it does when electric sparks are

* I do not mean to affirm that electricity never occasions combustion, the contrary of which is well known, but that a combination produced by it is not always the same with combustion.

passed through it. But this process has no resemblance to combustion. We see, however, that a certain temperature is capable of producing this change in air.

8. Several of the supporters and partial supporters are capable of combining with combustibles, without undergoing decomposition, or exhibiting the phenomena of combustion. In this manner the yellow oxide of gold and the white oxide of silver combine with ammonia; the red oxide of mercury with oxalic acid; and oximuriatic acid with ammonia. Thus also nitre and oximuriate of potash may be combined, or at least intimately mixed with several combustible bodies, as in gunpowder, &c. In all these compounds the oxygen of the supporter and the combustible retain the ingredients which render them susceptible of combustion; hence the compound is still combustible: And in consequence of the intimate combination of the component parts, the least alteration is apt to destroy the equilibrium which subsists between them; the consequence is, combustion and the formation of a new compound. Hence these compounds burn with amazing facility, not only when heated, but when triturated or struck smartly with a hammer. They have therefore received the name of *detonating* or *fulminating* bodies. Thus we have fulminating gold, fulminating silver, fulminating mercury, fulminating powder, &c.

Supporters, &c. may combine with combustibles without combustion, and produce *fulminating compounds*.

9. Such are the properties of the combustibles, the supporters, and the products; and such the phenomena which they exhibit when made to act upon each other.

If we compare together the *supporters* and the *products*, we shall find that they resemble each other in several respects. Both of them contain oxygen as an essential constituent part; both are capable of converting combustibles into products; and several of both combine with combustibles and with additional doses of oxygen. But they differ widely from each other in the phenomena which accompany their action on combustibles. The supporters convert these bodies into products, and combustion, or the emission of heat and light at the same time, takes place; whereas the products convert combustibles into products without any such emission. Now, as the ultimate change produced upon combustibles by both these sets of bodies is the same, and as the substance which combines with the combustibles is in both cases the same, namely oxygen, we

Supporters and products resemble in many respects;

but they differ widely in their effect on combustibles. The former only produce combustion.

The oxygen of supporters contains caloric.

Combustibles and products also resemble each other :

But the former emit fire when they combine with oxygen.

Combustibles contain light.

When supporters and combustibles combine, the caloric and light fly off in the combination called fire.

Combustion will not ensue if either of these in-

must conclude that this oxygen in the supporters contains something which the oxygen of the products wants, something which separates during the passage of the oxygen from the product to the combustible, and occasions the combustion, or emission of fire, which accompanies this passage. The oxygen of supporters then contains some ingredient which the oxygen of products wants. Many circumstances concur to render it probable that this ingredient is *caloric*.

The *combustibles* and the *products* also resemble each other in several respects. Both of them contain the same or a similar base; both frequently combine with combustibles, and likewise with oxygen; but they differ essentially in the phenomena which accompany their combination with oxygen. In the one case fire is emitted, in the other not. If we recollect that no substance but a combustible is capable of restoring combustibility to the base of a product, and that at its doing so it always loses its own combustibility; and if we recollect farther, that the base of a product does not exhibit the phenomena of combustion even when it combines with oxygen, we cannot avoid concluding, that all combustibles contain an ingredient which they lose when converted into products and that this loss contributes to the fire which makes its appearance during the conversion. Many circumstances contribute to render it probable that this ingredient is *light*.

If we suppose that the oxygen of supporters contains caloric as an essential ingredient, and that light is a component part of all combustibles, the phenomena of combustion above enumerated, numerous and intricate as they are, admit of an easy and obvious explanation. The component parts of the oxygen of supporters are two; namely, 1. a base, 2. caloric: The component parts of combustibles are likewise two; namely, 1. a base, 2. light. During combustion the base of the oxygen combines with the base of the combustible, and forms the product; while at the same time the caloric of the oxygen combines with the light of the combustible, and the compound flies off in the form of fire. Thus combustion is a double decomposition; the oxygen and combustible divide themselves each into two portions, which combine in pairs; the one compound is the *product*, and the other the *fire*, which escapes.

Hence the reason that the oxygen of products is unfit for combustion. It wants its caloric. Hence the reason that combustion

tion does not take place when oxygen combines with products or with the base of supporters. These bodies contain no light. The caloric of the oxygen of course is not separated, and no fire appears. And this oxygen still retaining its caloric, is capable of producing combustion whenever a body is presented which contains light, and whose base has an affinity for oxygen. Hence also the reason why a combustible alone can restore combustibility to the base of a product. In all such cases a double decomposition takes place. The oxygen of the product combines with the base of the combustible, while the light of the combustible combines with the base of the product. Thus when iron acts on water, the oxygen of the water combines with the base of the iron, while at the same time the light of the iron combines with the hydrogen of the water, and occasions its escape in the state of gas.

But the application of this theory to all the different phenomena described above, is so obvious, that it is needless to give any more examples. Let us rather inquire into the evidences which can be brought forward in its support.

10. Now as caloric and light are always emitted during combustion, it follows that they must have been previously component parts either of the combustible, or of the supporter, or of both.

They must therefore have previously existed in the combustible, the supporter, or both.

That the oxygen of the supporters contains either one or both of these substances, follows incontrovertibly from a fact already mentioned, namely, that the oxygen of products will not support combustion, while that of supporters will. Hence the oxygen of supporters must contain something which the oxygen of products wants, and this something must be caloric, or light, or both.

That the oxygen of some of the supporters at least contains caloric as an ingredient, has been proved, I think, in a satisfactory manner, by the experiments of Crawford, Lavoisier, and La Place. Thus the temperature of hot blooded animals is maintained by the decomposition of *air*. Now if the oxygen of one supporter contain caloric, the same ingredient must exist in the oxygen of every supporter, because all of them are obviously in the same state. Hence I conclude that the oxygen of every supporter contains caloric as an essential ingredient.

References to facts: Caloric.

The light emitted during combustion must either proceed from the combustible or the supporter. Now that it proceeds from the combustible must appear pretty obvious, if we recollect

lest

left that the colour of the light emitted during combustion varies, and that this variation usually depends, not upon the supporter, but upon the combustible. Thus carbonic acid burns with a blue flame, carbonated hydrogen with a white, and charcoal with a red; sulphur with a blue or violet, zinc with a greenish white, and phosphorus with a white.

Natural formation of combustibles and supporters: Vegetation.

The formation of combustibles in plants obviously requires the presence and agency of light; for when plants vegetate in the dark, their carbon is not increased, nor is any oily or resinous matter formed in them. The leaves of plants emit oxygen gas when exposed to the sun's rays, but never in the shade, or in the dark. Senebier has demonstrated that this emission is occasioned by the decomposition of carbonic acid. This acid, which is a product of combustion, is decomposed by the leaves of plants assisted by sunshine, and converted into oxygen gas and charcoal, a supporter and a combustible. This process is exactly the reverse of combustion, and must therefore restore the substances which had been lost during combustion; that is to say, caloric and light. But the sun's rays consist of these two bodies. Thus we see why plants require sunshine. A part of vegetation consists in decomposing, or *unburning*, products, and converting them into supporters and combustibles; but for such a conversion caloric and light are absolutely necessary.

The same effects by art.

Besides vegetation, we are acquainted with two other methods of unburning products, or of converting them into products and combustible; by exposing them, in certain circumstances, to the agency of *fire* or of *electricity*. The oxides of gold, silver, and mercury, when heated to redness, are decomposed, oxygen gas is emitted, and the pure metal remains behind. In this case the necessary caloric and light must be furnished by the fire; a circumstance which explains why such reductions always require a red heat. When carbonic acid is made to pass repeatedly over red-hot charcoal, it combines with a portion of charcoal, and is converted into carbonic oxide gas. If this gas be a combustible oxide, the base of the carbonic acid and its oxygen must have been supplied with light and caloric from the fire; but if it be a *partial combustible*, it is merely a compound of carbonic acid and charcoal: which of the two it is, remains still to be ascertained. Electricity decomposes water, and converts it into oxygen gas and hydrogen gas; it must therefore supply the heat and the light which these bodies lost when converted into a product.

These

These facts, together with the exact correspondence of the theory given above with the phenomena of combustion, render it so probable, that I have ventured to propose it as an additional step towards a full explanation of the theory of combustion. Every additional experiment has served to confirm it more and more*. It even throws light upon many phenomena which have been hitherto considered as altogether anomalous, as will be evident from the following observations.

Application of
this theory to
other facts.

II. In the year 1793, the associated Dutch chemists drew the attention of philosophers to a curious phenomenon which accompanies the formation of some of the sulphurets; a phenomenon previously noticed by Scheele; but which they first described in detail. When eight parts of copper filings, and three parts of flowers of sulphur are mixed together in a glass receiver, and the vessel placed upon burning coals, the mixture melts, a kind of explosion takes place, it becomes suddenly red hot, and a glow, like that of a piece of red hot charcoal fanned by bellows, rapidly pervades the whole. When this disappears, the mixture is found in the state of solid sulphuret of copper. Iron, lead, tin or zinc, may be substituted for copper. The experiment succeeds whether the vessel be filled with air, or with azotic, or hydrogen gas, or even with water or mercury. What is singular in this experiment is the glowing *red heat*, or the emission of *fire* which accompanies the combination of the sulphur and metal. This emission being the same which takes place during combustion, the process has been considered as a combustion, and stated as such by the German chemists, as an objection to Lavoisier's theory, which supposes that oxygen is a necessary agent in that process: while other philosophers have denied that this operation is a combustion, or that it has any resemblance to that process.

Ignition produced by the fusion of sulphur with a metal,

The same emission of caloric and light, or of *fire*, takes place when melted sulphur is made to combine with pot-ash, or with lime, in a crucible or glass tube, and likewise when

or of sulphur with potash or lime; or phosphorus with lime, &c.

* In the preceding enumeration of facts I have not taken notice of the modifications which the Lavoisierian theory has received from Hutton, Delametherie, Richter, and Brugnatelli; because I suppose them sufficiently known. Every one of these modifications agrees in some particulars with the theory given in this Paper, but differs from it in others.

melted phosphorus is made to combine with lime heated nearly to redness. In all probability barytes and strontian exhibit the same phenomenon when combined with melted sulphur or phosphorus; and some of the metals when combined with phosphorus. In general then the emission of *fire* accompanies the combination of melted sulphur and phosphorus, with several of the earths, fixed alkalies and metals, heated previously to a certain temperature.

Explanation.
Fused sulphur or phosphorus contain *caloric*: metal. &c. contain *light*. They become *solid* in combination and give out *caloric* and *light*; i. e. *fire*.

To explain the phenomenon we have only to recollect, 1. That the sulphur and phosphorus are in the melted state, and therefore contain *caloric* as an ingredient. 2. That the alkalies, earths and metals which produce the phenomenon in question, contain *light* as an essential ingredient. 3. That the sulphuret or phosphuret formed is always in a solid state; these three points once established, the process admits of a very simple explanation. The sulphur or phosphorus combines with the base of the metal, earth or alkali; while at the same time the *caloric* to which the sulphur or phosphorus owed its fluidity, combines with the *light* of the metal, earth, or alkali, and the compound flies off under the form of *fire*.

The process resembles combustion except in its product;

Thus the process is exactly the same with combustion, excepting as far as regards the product. The melted sulphur or phosphorus acts the part of the *supporter*, while the metal, earth or alkali occupy the place of the *combustible*. The first furnishes *caloric*, the second *light*, while the base of each combines together. Hence we see that the base of sulphurets and phosphurets resembles the base of products in being destitute of *light*, the formation of these bodies exhibiting the separation of *fire* like *combustion*, but the product differing from a product of combustion in being destitute of oxygen, we may distinguish the process by the title of *semi-combustion*; indicating by the term, that it possesses one half of the characteristic marks of combustion, but is destitute of the other half.

termed semi-combustion.

Facts to shew that potash and lime contain *light*.

The only part of this theory which requires proof is, that *light* is a component part of the earths and alkalies. But as potash and lime are the only bodies of that nature, which I am certain to be capable of exhibiting the phenomena of semi-combustion, the proofs must of necessity be confined to them. Now that *lime* contains *light* as a component part has been long known. Meyer and Pelletier observed long ago, that when water is poured upon quicklime not only heat but *light* is emitted. *Light* is emitted also abundantly when sulphuric

acid is poured upon lime*. In both cases a *femi-combustion* takes place. The water and the acid being solidified give out *caloric*, while the quicklime gives out *light*; that lime during its calcination combines with light, and that light is a component part of quicklime is demonstrated by the following experiment, for which we are indebted to Scheele.

It is well known that fluor spar (native fluat of lime) has the property of phosphorescing strongly when heated, but that the experiment does not succeed twice with the same specimen. After it has been once heated sufficiently, no subsequent heat will cause it to phosphorate. Now phosphorescence is merely the emission of light, light of course is a component part of fluor spar, and heat has the property of separating it. But the phosphorescing quality of the spar may be again recovered to it, or which is the same thing, the light which the spar had lost may be restored by the following process. Decompose the fluat of lime by sulphuric acid, and preserve the fluorine acid separated. Boil the sulphate of lime thus formed with a sufficient quantity of carbonate of soda; a double decomposition takes place; sulphate of soda remains in solution, and carbonate of lime precipitates. Calcine this precipitate in a crucible till it is reduced to quicklime, and combine it with the fluorine acid to which it was formerly united. The fluor spar thus regenerated phosphoresces as at first. Hence the lime during its calcination must have combined with light.

That potash contains light, may be proved in the same manner as the existence of that body in quicklime. Dizé has shown that much light is emitted when sulphuric acid is poured upon potash, but more when it is poured on the carbonate of potash. Now as potash is deprived of its carbonic acid by lime, it is obvious that the process must be a double decomposition; the base of the lime combines with carbonic acid, while its light combines with the potash.

These remarks on semi-combustion might easily be extended much farther. For it is obvious, that whenever a liquid combines with a solid containing light, and the product is a solid body, something analogous to semi-combustion must take place. Hence the reason why water increases the violence of combustion when thrown sparingly into a common fire.

* Dizé Jour. de Phys. 49, 177.

OBSERVATIONS ON SPONTANEOUS LIGHT.

IX.

*Continuation of the Experiments and Observations on the Light
as spontaneously emitted from various Bodies; with some
Experiments and Observations on solar Light, when imbibed
by CANTON'S Phosphorus. By NATHANIEL HULME,
M. D. F. R. S. and A. S.*

(Concluded from page 40.)

§ 5.

Effects of carbonic Acid Gas or fixed Air on spontaneous
Light.*

EXPERIMENTS.

1. **AT** 10 P. M. a piece of fresh herring, weighing three drams, was suspended in a wide-mouthed ten-ounce phial, filled with carbonic acid gas, and closed with a cork and bladder. It was retained there for three successive nights; but emitted no light.

spontaneous
light; but it re-
vives by com-
mon air.

Exp. 2. The same experiment was made with a piece of herring, which was beginning to be luminous. On the next evening, the illumination was found to be extinct: nevertheless the herring was still kept in the gas for three nights longer, but did not become lucid.

Exp. 3. At 7 P. M. a piece of fresh mackerel was introduced above water, into a wide-mouthed bottle, holding 24 ounces, which was completely filled with carbonic acid gas, and supported by a tea-faucer that held about three ounces of water. On the second night it was dark, and continued the same on the third. It was then exposed to the influence of atmospherical air, and, on the next evening, it was pretty luminous, and likewise on the succeeding night.

Exp. 4. At 9 P. M. a cork, smeared with the luminous matter of a mackerel, was put into a five-ounce wide-mouthed phial, filled with carbonic acid gas, and then closed with a glass

* This gas was obtained from powdered chalk, or marble, and diluted sulphuric acid.

stopple.

floppe. It continued to shine pretty vividly for some little time; then the light gradually diminished, so that at twelve, only a small spark remained.

Exp. 5. At 10 P. M. another cork, illuminated with mackerel-light, was introduced above water, into 24 ounces of the gas; and its light was nearly extinct at twelve.

Exp. 6. At 8 P. M. a fragment of shining wood was put above water, into 24 ounces of the gas; and it had not been long there before the light disappeared. It was then taken out, and exposed to the action of atmospheric air, when its shining property soon returned.

Exp. 7. Another fragment of brightly shining wood was introduced above water, into the same quantity of the gas, at 10 P. M. and the light was extinguished in the space of an hour. After this, it was exposed to the open air, and the light gradually revived.

Exp. 8. At 8 P. M. a luminous dead glow-worm was put above water into the gas; its glowing appearance gradually faded, and in a short time became quite invisible. It was then taken out, and the light, by degrees, re-appeared as vivid as before.

OBSERVATION.

This gas, we find, has also an extinguishing property, with respect to spontaneous light: but, in general, the light returns, if the object of experiment be taken out, and exposed to the open air.

§ 6

The Effects of sulphurated hydrogen Gas on spontaneous Light.*

EXPERIMENTS.

Exp. 1. At noon, a piece of a very fresh mackerel, with a bright eye, was introduced above water, into 24 ounces of this gas, and was retained therein for three successive evenings, without emitting any light. It was then exposed to atmospheric air; yet it continued dark on the two following

Sulphurated hydrogen extinguishes spontaneous light more effectually and more permanently than carbonic acid.

* This gas was obtained from sulphuret of potash and diluted muriatic acid.

nights;

nights: but, on the third, it was very luminous, and remained so on the fourth and fifth.

Exp. 2. The same experiment was then made with a piece of fresh herring, which was also kept in the above gas, for about three nights, without being luminous. After exposure to common air, it did not emit any light during the first 24 hours. However, on the subsequent night, it began to shine, had a very bright light on the following evening, and continued shining for several succeeding nights.

Exp. 3. A cork, smeared with the luminous matter of a herring, was put above water, into 24 ounces of the gas; and the light was extinguished in less than an hour. The experiment was repeated in the same gas, and with the same result.

Exp. 4. A cork, illuminated with mackerel light, was introduced into the same quantity of gas; and was dark in half an hour.

Exp. 5. A fragment of shining wood, being put into the gas, became dark in eight minutes. A second piece became dark in five minutes. They were then taken out, and continued dark all that evening. On the next evening, one of the pieces was uncommonly lucid.

Exp. 6. At 10 P. M. another fragment of brightly shining wood was introduced above water, into 24 ounces of the gas, and was extinct at eleven. It was then exposed to the open air; but there was no return of light that evening. On the following night, it was found pretty luminous.

Exp. 7. A finely shining dead glow-worm was next put above water, into this gas, and its light was quickly extinguished. In a second experiment, in the same gas, the light was much slower in its extinction. In both instances, after the insect was withdrawn, and placed in atmospheric air, the light gradually revived.

OBSERVATION.

It is apparent, by these experiments, that sulphurated hydrogen gas extinguishes spontaneous light much sooner than carbonic acid gas, and that, in general, the light returns much more slowly, when the subject is exposed to atmospheric air.

§ 7.

*The Effects of nitrous Gas * on spontaneous Light.*

EXPERIMENTS.

Exp. 1. A piece of fresh herring was introduced above water, into this gas, at 3 P. M. and remained there four nights, without emitting any light: it was then withdrawn, and exposed to common air, for the space of three nights; but did not become lucid.

Exp. 2. The same experiment was made with a piece of herring beginning to be luminous; but its light was gradually extinguished: it was detained in the gas for three nights, and taken out dark. It was then exposed to the open air, for the three subsequent nights; but its shining appearance did not return.

Exp. 3. A cork with luminous matter, introduced above water, into this species of gas, had its light, in general, extinguished in from 10 to 30 minutes; and, when taken into common air, its light very seldom re-appeared.

Exp. 4. Fragments of shining wood, above water, in nitrous gas, were likewise commonly rendered dark in a very short space of time, as in three or four minutes; sometimes a fragment, if uncommonly luminous, would not be extinguished in less than six or eight minutes; and very seldom would the light revive, on exposing the wood to atmospheric air.

Exp. 5. A dead shining glow-worm being put above water, into this gas, its light was quickly extinguished; but, after the insect was taken into the common atmosphere, the light gradually returned. The experiment was thrice repeated, and with the same result.

OBSERVATION.

This species of gas, we observe to have totally prevented the emission of light, and to have quickly extinguished that which had been emitted: likewise that the luminous objects which had been under its influence, (except the glow-worm) did not experience a revival of their light, when taken out, and kept for some time in common air.

* This gas was obtained from copper and diluted nitrous acid.

§ 8.

The Effects of a Vacuum on spontaneous Light.

EXPERIMENTS,

The light is extinguished in vacuo, but beautifully restored by admission of air.

Exp. 1. A piece of shining wood, of a moderate size, was put under the receiver of an air-pump, in a dark room; in proportion as the air was extracted, the light was gradually extinguished, and at last reduced to a mere point, just visible, owing most probably to a small residuum of air, which is always left, even in the most perfect machine. Fresh air was then leisurely admitted, and the light was immediately revived in a very beautiful manner. This experiment was frequently repeated, and always with the like effect.

Exp. 2. Some luminous matter of a herring, uncommonly bright, was smeared upon a piece of red blotting paper, and then submitted to the operation of the air-pump. The light became fainter and fainter, as the inclosed air was withdrawn, and at last nearly vanished; but brightened up as before, on the influx of fresh air. The experiment was repeated, and with the same result.

SECTION XII.

Experiments and Observations on solar Light, when imbibed by Canton's Phosphorus.

§ 1.

*The Effects of Heat on imbibed solar Light.**I. The imbibed Light is rendered more vivid by a moderate Degree of Heat.*

EXPERIMENTS,

Canton's phosphorus shines more when heated.

Exp. 1. Having prepared some Canton's phosphorus, and exposed it to the light of the sun, it was carried into the dark laboratory, to separate the illuminated parts from those that remained dark. In doing which, some luminous fragments were placed upon the palm of the hand, and retained there for some time, when it was observed, that the warmth of the hand considerably increased the degree of light.

Exp. 2.

Exp. 2. Some fragments of this illuminated phosphorus were put into a small phial, which was then closed with a cork, and suspended, by a string, in a quart of water heated to about 126° ; by these means, the light was rendered much more vivid than before.

Exp. 3. Some other pieces of the illuminated phosphorus were dropped separately into a glass tube 32 inches long, and $\frac{7}{8}$ bore, filled with water at about 120° . The light of each piece became exceedingly bright, as soon as it entered the hot water; and they all descended, very luminous, from the top to the bottom, some quickly and others slowly, according to their gravity, making a very pleasing experiment.

Exp. 4. A large wooden bowl, about 12 inches wide, was next filled with water heated to about 110° , and then a quantity of illuminated phosphorus, partly in the form of powder, and partly in pieces of different magnitudes, was scattered over the whole surface of the water; all which pieces fell, with increased splendour, to the bottom, where they preserved their light for some time,

II. *The imbibed Light is extinguished by a great Degree of Heat.* but too much heat extinguishes it,

Exp. 5. Some fragments of the phosphorus, rendered luminous, were exposed to a greater degree of heat, namely, by casting them into a tin vessel containing two pints of boiling water. They flashed with increased light, as soon as they came in contact with the water, fell precipitately to the bottom, in a lucid state, and then were gradually extinguished.

Exp. 6. In which the degree of heat was still increased. A small bar of iron, of about an inch square, was made red-hot, and laid horizontally in the laboratory, until, by cooling, it nearly ceased to shine. Some pieces of illuminated phosphorus were then put upon it in succession, and the light, in a moment, glowed with uncommon lustre, but was quickly after totally extinguished*.

* Solar light, when received merely on a piece of white paper, may also be rendered more luminous by heat, and then extinguished by it, as appears from an experiment made by the late Mr. B. Wilton, whose book on phosphori I had not seen before this Paper was drawn up.

III. *The*

Latent light is
excluded from
Canton's phos-
phorus by heat.

III. *The imbibed Light, after being in a latent State, is excited and rendered luminous by the Agency of Heat.*

Exp. 7. Some small pieces of the phosphorus, after having been illuminated, were deposited in the laboratory; when the light by degrees faded away, and became totally invifible. They were kept in this dark state for the space of ten days, and then placed one after another upon a heated bar of iron, as in the last experiment, upon which they quickly became exceedingly luminous.

From an experiment made by the ingenious Mr. Canton, I obferve, that fome of his phosphorus, contained in glafs balls hermetically fealed, and heated in the above manner, gave a confiderable degree of light, after it had been kept in a state of darknefs more than fix months. Phil. Tranf. Vol. LVIII. page 342.

§ 2.

The Effects of Cold on imbibed Light.

EXPERIMENT.

Cold extinguifhes
the solar light
of Canton's
phosphorus, &c.

About 15 grains of the phosphorus were put into a half-ounce phial, containing two drams of cold pump water, that had been deprived of its air by boiling. The phial was then corked, and expofed for fome time to solar light, whereby the phosphorus became finely illuminated. In this ftate, it was immediately put into a frigorific mixture, compofed of fnow and fea falt, and retained there about 30 or 40 minutes, when it was taken out, and the light found to be totally extinguifhed. The phial was then placed in fome water, at about 60° temperature, and the light gradually revived, and became as brilliant as before it had been expofed to the cold. This experiment was frequently repeated, and always with the fame refult.

I cannot but remark, that in the courfe of experiments on this fubject, the fuperior power of solar over that of fpontaneous light was very apparent. For, the firft trials being made in fmall phials, containing only atmofpheric air with the phosphorus, the light was with fome difficulty totally extinguifhed; and, after the phials were taken out of the frigorific mixture,

mixture, the temperature of the laboratory would commonly soon revive the light, which rendered the experiments not altogether satisfactory. Finding it thus somewhat difficult to extinguish solar light *in air*, recourse was had *to water*, in the manner above described. This answered perfectly well; for the water, when frozen, gave a substantial body, as it were, to the imbibed light of the phosphorus, so as to enable it to retain the excess of cold arising from the frigorific mixture; thereby making the experiments quite satisfactory. When the phosphorus was thus surrounded by ice, only a few minutes stay in the frigorific mixture would generally be sufficient for a total extinction.

OBSERVATION.

From these experiments, compared with those recited in my former Paper on spontaneous light, it appears that solar light, when imbibed by Canton's phosphorus, is subject to the same laws, with respect to heat and cold, as the spontaneous light of fishes, rotten wood, and glow-worms.

P. S. In these experiments with solar light, the phosphorus was sometimes exposed to the direct rays of the sun, at other times to common day-light, in a northern aspect; and it was remarked, that it became somewhat more luminous by mere day-light, than by the rays of the sun.

It may also be proper to observe, that the above experiments were made with an improved preparation of Canton's phosphorus. This improvement, which was first made by Dr. Higgins, consists in omitting the pulverization of the shells.

Improved preparation of Canton's phosphorus by Dr. Higgins.

His method was, after calcining the oyster-shells, to put the pieces, both great and small, in layers, into a crucible furnished with a cover, and to sprinkle flowers of sulphur between each layer. After they had remained some time in the furnace, they were taken out, suffered to cool, and then kept in a large bottle with a glass stopple. For this communication, I am indebted to Mr. Lewis of Holborn, near Southampton-street, who has an extraordinary dark room, where, at times, he amuses his friends with some beautiful appearances, arising from solar light imbibed by phosphorus prepared as above directed. A still further improvement of this phosphorus, it appears to me, may be made by substituting precipitated sulphur for the flowers of sulphur; and the experiments of this section were chiefly made with phosphorus so prepared.

Dark room of Mr. Lewis.

X. Description

X.

*Description of a Lamp upon ARGAND'S Principle, with Improvements, in which the Oil is maintained at the same Level by the constant Action of a Pump. By Citizens CARCEL and CAREAU *.*

Excellence of
the lamp of Ar-
gand.

LAMPS with a double current of air are among the inventions of the eighteenth century, which are most honourable to the industry of France, and of which the general use sufficiently bespeaks the value. But it is not enough that a great light should be produced without smell or smoke, but an object of nearly equal utility consists in producing the light steadily, with economy, in the most advantageous form, and best adapted to economical purposes.

Improved by
Carcel and Ca-
reau.

Citizens Carcel and Careau have succeeded in this object. We shall not dwell upon the obstacles they must have met with in arriving at the degree of perfection they have obtained; it will be sufficient to shew the value of their invention, if we can prove that it surpasses every thing of the kind which has yet been exhibited. This lamp has the double advantage of exhibiting all the good qualities of the lamp we before possessed, without their inconveniences. The nozzle undergoes no alteration by heat, neither is the wick destroyed, but almost constantly preserves its whiteness.

Inconveniences
of lamps of the
usual construc-
tion.

The oil in ordinary lamps is liable to flow out by its expansion when heated; but in this mechanical lamp it constantly preserves its level. By dispensing with the reservoirs of oil in those lamps which are called fountain lamps, these inventors have succeeded in affording light which is not shaded on any side. In this respect the lamp is very economical, because nearly half the light of a fountain lamp must necessarily be intercepted by the receiver, which requires to be placed above the level. But in these the reservoir is in the foot, which renders them more portable; and from this circumstance, as well as the other advantages of their construction, there is no dan-

* From Les Annales des Arts & Manufactures, vi. 269. I am much obliged to my correspondent C. D. for directing my attention to this article.

ger of spilling the oil by inclining it, which is an inconvenience of great magnitude, and hitherto constituting one of the greatest objections to the lamps in use.

In other lamps the flame varies in its intensity commonly in about two hours; but in the mechanical lamp it constantly preserves the same supply and the same brilliancy.

The mechanism adapted to the foot in order to cause the oil to ascend, has been reduced to the greatest simplicity; it is firm and durable, and has no communication with the oil of the reservoir.

This lamp may be used in distillations and chemical preparations, as well as in culinary purposes; and in general we cannot do better than transcribe the report made to the National Institute by Guyton, Morveau, and Charles.

"The Class having charged us to examine the mechanical lamp presented by the Citizens Carcel and Careau, at the sitting of the 21st of last month, as a means of adding a new degree of perfection to lamps with an inner current of air, as well as with regard to the intensity of light as to economy and the convenience of daily use.

"Lamps excited by an interior current of air, of which the invention belongs to Citizen Argant, and which were announced for the first time in February 1784*, have produced a revolution in the art of illuminating which time has served only to confirm, as it does all those which, being founded on true principles, receive the daily sanction of experience.

"A short time afterwards Citizen Lange thought of contracting the glass chimney, so as to direct the external current of air nearer to the flame, by which means he determined a still more complete combustion of the oil, and produced a more brilliant light, without either smoke or smell. The union of these inventions seemed to have exhausted the subject, but Citizens Carcel and Careau apprehended that it was possible to render the lamps still more perfect. They considered that the best lamps of this description do not constantly afford the same intensity of light, because the wick not being constantly and alike supplied with oil, is subject to become charred; that it is necessary either to raise it beyond the proper elevation, or to trim the lamp again after some hours; and lastly, that the glass

* Journal de Physique for that month, page 159.

chimney not being capable of being made to that degree of accuracy as to afford the contraction always at the same height, the effect of this contrivance could not be the best possible, excepting when the accidental concurrence of circumstances might render it so.

“ At the same time that they were busied in remedying those inconveniences, they did not neglect the means of rendering the lamp more economical, convenient, and portable; without fear of spilling the oil; without any considerable effect upon the flame by motion; without any interception of the light from a reservoir; and with the addition of an agreeable and ornamental form.

“ The account resulting from our examination will enable the Class to judge of the difficulties they have had to vanquish, the ingenious processes they have used, and the success which has crowned their industry.

Experiments
with the new
lamp.

“ The mechanical lamp was lighted before the Class for a short time. They saw the brilliant light it afforded, and compared that light with the lamps which usually illuminate their place of meeting. It continued absolutely the same five hours after it had been lighted, though the wick had not been altered or touched.

Compared with
the common,
it gives more
than twice the
light.

“ In order to determine more exactly the intensity of the light, it was placed at such a distance that a body interposed between its light produced a shadow of the same obscurity as that of a common Argand's lamp. The distances were—from the mechanical lamp forty-six decimeters, and from the common lamp thirty-three decimeters, which, by the square of the distances, gives the ratio of 2116 to 1089, or 100 to $48\frac{1}{4}$, for the intensities of the light.

“ The common lamps of this construction not always affording a light perfectly uniform, and the glass chimney of that which we employed not being of the most favourable dimensions, we thought it proper to endeavour to obtain a confirmation of this ratio by the comparison with candles, of which the light is less subject to vary.

Comparison with
candles.

“ Six candles (I suppose of wax), of the weight of one hectogram (five to the French pound), were arranged in such a manner that their flames could not mutually intercept each other. The shadow produced by the interposition of an opaque body received on a card, was found to be similar to that of the mechanical.

mechanical lamp when the candles were brought to the distance of 566 centimeters, at the same time that the mechanical lamp was drawn back to 785. The squares of these numbers give the ratio of 100 to 52 nearly. If instead of the latter term we put the quotient of its division by the number of candles, we see that the light of the mechanical lamp is to that of one candle as 100 to $11\frac{1}{2}$, or it would require eleven candles and a half to give the same light.

“As this lamp may be used for the domestic purposes of affording heat as well as for the operations of chemistry even in the dry way, it becomes interesting to ascertain the degree of heat it was capable of communicating to vessels placed above its glass chimney. For this purpose we took a pyrometric piece of Wedgwood not baked; it was included between two very thin capsules of platina, and placed on a support, first at four centimeters, where, at the end of half an hour, the contraction was three degrees of the pyrometric scale; after which it was again placed at the distance of fifteen millimeters only from the upper extremity of the chimney, where, after having been kept for two hours, it was found to have passed the seventh degree, which, according to the table of correspondence of Wedgwood, would indicate 505 degrees of the centigrade thermometer.

“A tube of glass of five millimeters, or two lines, in diameter, of the weight of thirty-seven decigrams, or sixty-six grains, the cube decimeter, was easily bended over the chimney, that is to say, at eight centimeters, or two inches, &c. from the upper extremity of the flame. Tin, placed in a small crucible of kaolin, upon a support of the same height, flowed in less than seven or eight minutes. Five grams, or eighty grains, of antimony in small fragments, afforded in a similar crucible, at the end of an hour, a degree of fusion sufficiently advanced to round the lower part of the button, and it is known that the lowest estimation of the heat required to fuse this metal is 431 degrees.

“The consumption of oil was determined by several trials, one of which was continued for seven hours: It varied very little; the mean term was 34,648 grams per hour. ($1\frac{1}{4}$ oz. avoird.)

“With regard to the operative means by which these effects are obtained, we shall remark, first, the disposition of the chimney, which can be raised or lowered at pleasure by turning the cylinder

cylinder which supports it on its screw, which has nine threads by which means the true situation to produce the best effect is obtained, whatever may be the difference of size between one glass and another. The screws are pewter, but the reporters observe, that it may be better to make one of them iron.

"The advantages of raising the oil by a pump moved by a spring are real, and prevent the wick from becoming charred for want of a constant supply of oil; besides which, as the flame is kept at a distance from the socket, it can neither calcine the metal nor deposit that crust of hardened oil which so soon alters the effects of other lamps.

The Commissaries express their approbation of the expedient by which the oil is prevented from leaking through and arriving at the mechanism, and state, that they examined the mechanism, which they find to be well calculated to produce the desired effect, and to prove as durable as could be wished.

Illumination
judged by read-
ing a book.

The brilliancy of effect is spoken of in high terms of approbation. They found that when a white gauze shade was put over the lamp, they could read in the *Anacharsis* of *Didot* at the distance of 78 decimeters, or about 24 feet; and that they could read in the same book at the distance of 89 decimeters with the flame uncovered. They do not mention the type of the book.

Blue chimney.

With a chimney of blue glass they could read at 72 decimeters.

When a ground glass was placed before the flame, they could read at the distance of 75 decimeters. A candle placed behind the same glass, gave a light by which it was difficult to read at 23 decimeters.

Particular de-
scription of the
construction of
the lamp.

In addition to the report of the Commission of the Institute, the Editor of *Les Annales des Arts* gives a more minute description, with an accurate engraving, which is presented to the reader in plate IX.

Fig. 1. View of the under part of the movement of the lamp: *aa* the lower plate of brass, which, with the superior plate connected by four brass pillars, forms the frame of the movement: *b*, ratchet wheel for winding or setting up the spring: It is pinned upon the square of the barrel arbor, and is kept in its place by the click and back spring *c*: The two holes in the face of the wheel are intended to receive the points of a key for winding it up: *d*, a plate which receives the

the pivots: *e*, large wheel, the pinion of which is driven by the wheel of the barrel: *ffff*, ends of the pillars: *h*, the fly and its endless screw.

Particular description of the construction of the lamp.

Fig. 2. Plan of the upper side of the movement: *i*, upper brass plate: *k*, the bottom of the reservoir of tin: *lll*, connecting pillars: *m m*, a circle with its screws, which serves to support the pump and its apparatus: *n*, body of the double force pump: *o*, aperture through which the oil rises through the ascending pipe: The elevation of this pipe is seen at *o*, fig. 3: *p*, the double arm of the pistons of the pump: *q q*, pump rods.

Fig. 3. Lateral elevation of the mechanism: *ss*, the plate which covers the body of the lamp, fixed by four screws, which at the same time connect the lower plate *u*: This plate is raised up in the drawing instead of being screwed down, in order to give a clearer idea of the connection. The middle piece *t* is perforated by two cylindrical holes, in which the pistons move which drive the oil. The piece *t* is also perforated above and beneath, in order to admit of two conical valves opening upwards into each cavity or pump barrel. The lower valve in each suffers the oil to rise into the barrel as the piston is withdrawn, and the upper suffers it to pass upwards when the forcing stroke takes place. The pieces *s*, *t*, and *u*, are secured by a piece of leather put between them before they are screwed together.

The dotted line *vv* denotes part of the reservoir of oil: *w*, part of the lower plate *m*: *x*, one of the pillars: *y*, the barrel containing the spring: *z*, pinion of the large wheel: *A*, pinion of the wheel of the endless screw: *B*, the wheel which drives a crank that works the pumps: *C*, the leading piece which acts on the lever: *D*, pinion of the wheel *B*. *E*, middle wheel, taking in the pinion *D*, and moved by its own pinion, which takes in the great wheel *e*: *F*, lever that works the arms of the pump.

Fig. 4. Elevation of the natural size of the glass globe: 1, A pivot of steel which passes through the glass globe to communicate with the arms of the pump. The lower part of this pivot is square, in order to receive the lever of oscillation; the upper part is round, and has the arms of the pump driven tight upon it. 2, A brass socket in which the pivot 1, 3 turns, and is prevented from descending by a shoulder. The glass

Particular description of the construction of the lamp.

globe, or pearl-shaped vessel, is blown from a piece of barometer tube, and is connected in the socket by sealing wax (or lac), which is insoluble in oil. Citizen Carcel found a great difficulty in preventing the leakage of the oil to the works, which he has obviated by filling the glass vessel *r* with molasses thickened by heat. This fluid is not acted on by oil, and answers the purpose very well, without impeding the motion.

Fig. 5. Elevation of a chandelier to which the mechanism is adjusted. *G*, the foot containing the movement: *H*, reservoir containing oil, and the pump: *I*, the stem of the chandelier, forming part of the reservoir, and traversed by the tube of ascension *o*, which supplies the wick with oil: *K*, the apparatus of the lamp, having apertures through which the air enters. Fig. 6. Another more elegant figure proposed by C. O'Reilly.

The number of the wheels are as follow: Barrel wheel, 108 teeth: It drives a pinion of 12 on the arbor of *c*, which has 84 teeth. The middle wheel *E* has 96 teeth, and a pinion of 12. The crank wheel has 90 teeth and a pinion of 8. The last or endless screw wheel has 20 teeth, and a pinion of 12. And the screw has two threads.

The movement will go twelve hours without winding up.

The use of the lamp is simple and easy. The wheel-work is either locked or set at liberty at pleasure by a stop. In the regular process, the wick is to be first trimmed; then the mechanism is to be wound up; and in the next place, the oil is to be poured in. When the stop is disengaged, the oil is seen to rise up to the wick, and this is the proper time to light it. When it is no longer wanted, the movement is to be stopped of course, at the same time that the lamp is extinguished.

XI.

Note upon a peculiar vegetable Principle contained in Coffee. By RICHARD CHENEVIX, Esq. F. R. S. M. R. I. A. From the Author. May 24, 1802.

Coffee heated in water, then filtered and evaporated, IN a vessel calculated to confine the vapour of water, I heated a considerable portion of that liquid upon about a pound of raw coffee imported directly from Martinico, and of

of the quality of which I was well assured. I then filtered the liquor, and reduced it nearly to dryness, in a glass evaporating dish, at a gentle heat. By this means I obtained a small quantity of a clear yellow residuum, like the most transparent horn, and of the consistence of honey. This residuum did not deliquesce, or seem to be subject to change, by exposure to the atmosphere. It was soluble in alcohol. It did not manifest either acid or alkaline properties. By some experiments I perceived it to be a substance differing essentially from all the vegetable principles with which I was acquainted; and, finding that I could obtain it pure by a method which Proust used to procure tannin, I proceeded in the following manner:

I poured a solution of muriate of tin into some water which had been made to boil upon coffee, and obtained a precipitate, which I collected upon a filter, and washed. I then put it into water, and caused a current of sulphurated hydrogen gas to pass slowly through the liquor. By this process the oxide of tin combined with the sulphurated hydrogen gas, and the substance originally contained in the coffee, but which, as I shall immediately shew, had combined with the metallic oxide, was disengaged, and remained in the liquor, while the hydrogenized sulphuret of tin was precipitated. It then remained only to evaporate the liquor to obtain the vegetable principle. In this state it exhibited nearly the same appearance as before it had been combined with the oxide of tin, but seemed of a purer, lighter colour, and more clear and transparent, being freed, as I suppose, from all extractive or other matter.

Imagining it now to be sufficiently pure, I dissolved it in a very small proportion of water, and examined it chemically.

The solution was of a bright horn colour; had a bitter taste, though not unpleasing. It was neither acid nor alkaline.

Solution of potash, of soda, or of ammonia, poured into the liquor, changed its colour to a bright garnet red.

Nitric acid produced a similar effect.

Very concentrate solutions of the alkaline carbonates did not cause a precipitate, as in a solution of tannin.

Sulphuric acid became of a dirty brown colour with the solution, but no other change was apparent.

With muriatic, phosphoric, and the vegetable acids, there was no change but what would naturally result from a mixture of the colours of both liquors.

gave a clear yellow residuum,

essentially different from other vegetable products.

It was precipitated by muriate of tin; and the tin thrown down by sulph. hydrogen.

The disengaged new vegetable principle was thus obtained

Its habitudes. Soluble in alcohol, and in water: bitter taste.

Alkalies turn it red;

as did nitric acid.

No precipitate by alkaline carbonates.

Sulphuric acid, little change.

Other acids, none.

Solution of iron; beautiful green precipitate. With any solution of iron in which there was not excess of acid, the liquor passed to a beautiful green; and, if it was concentrated, there was a green precipitate. Salts formed of the red oxide of iron succeeded the best; and the reciprocal action of this principle and iron is almost as delicate as that of either gallic acid or tannin and iron.

Muriate of tin, yellowish precip. With muriate of tin there was a very abundant yellowish precipitate, which was a combination of the new vegetable principle with oxide of tin. Both this precipitate and that with iron are soluble in all the acids, and the liquors lose their colour.

Waters of lime, barytes, strontian. Lime-water did not cause any precipitate in this liquor, nor did strontia-water. Barytes-water gave a fawn coloured precipitate. With lime-water tannin gave a bluish-green precipitate; and nearly the same with strontia-water, as also with barytes-water.

Gelatine gave no precipitate. A solution of gelatine did not give any precipitate with this vegetable principle. The effect of tannin upon gelatine is well known.

Hence this principle is different from any other. By these experiments it is proved that the principle bears sufficient characters to distinguish it from tannin, or any other vegetable principle with which we are acquainted. The only property which it possesses in common with tannin, is its affinity for oxide of tin, while it is clearly distinct from tannin in every other point.

Coffee does not exhibit tannin till roasted. It is evident that coffee, before it is roasted, does not contain tannin. A solution of gelatin poured into a decoction of well roasted coffee, gives, however, an immediate precipitate; and the precipitate is the combination of tannin with gelatin.

Whether this new principle, while in the vegetable, may not be converted into tannin by heat. Messrs. Proust, Seguin, and Davy, have observed, that heat develops the tannin principle in many vegetables. In a commercial point of view, it might be advantageous to examine, whether those vegetables are not such as, before they are heated, contain this new principle. Although I did not perceive that the principle, when insulated from the entire vegetable, was converted by heat into tannin; yet the presence of the other component parts of the vegetable may influence the distribution of elements in such a manner as to produce combinations different from what the separate principles will afford. I have not had an opportunity of extending these researches any further in the vegetable kingdom.

XII.

Account of some Experiments performed upon a Scale of considerable Magnitude, and principally by the Agency of Frost, to produce Sulphate of Soda, Carbonate of Magnesia, and Muriate of Ammonia, from Sulphate of Magnesia, Carbonate of Ammonia, and Muriate of Soda. By H. CAMPBELL, M. D. From the Author.

THE selected saline substances were, Epsom salt, or the sulphate of magnesia; table salt, or the muriate of soda; and concrete hartshorn drops, or ammoniacal carbonate. Of the proper mixtures, calculation was formed upon a supposed acquaintance with the table of agents entering into the composition of each body.

TABLE.

	Acid.	Alkaline Earth.	Water.
Sulphate of magnesia,	33 sulphuric	19 magn.	48
Muriate of soda,	52 muriatic	42 —	6
Ammoniacal carbonate	45 carb. acid	43 —	12

The expected and unavoidable products were,

	Acid.	Alkali.	Water.
Glauber salts, or sulphate of soda, containing - - -	27 sulph.	15 magn.	58
Salited or muriated magnesia, containing - - -	34 mur.	41 —	25

And upon the addition of the ammoniacal carbonate to the salited magnesia,

Carbonate of magnesia - -	30 carbon	48 earth	22
Muriate of ammoniac - -	52 —	40 —	8

The salited magnesia must only be considered as an intermediate product.

To avoid the detail of various experiments, those found to be most practicable will be only related, connected with the most practicable apparatus.

In iron pans of 150 gallons each, due proportions of sulphate of magnesia and muriate of soda were put in water in proportion to the laws of solubility and crystallization. The saline liquor was pumped into troughs lined with tinned iron plate. The troughs were thirty feet long, one foot wide, and three inches

Sulphate of magnesia and mur. soda gave sulph. soda and mur. mag. To the latter was added carbonate of ammonia, and the products were, muriate of ammonia and carb. of magnesia.

Detail of the experiment.

inches deep. Some of the liquor was exposed in butcher's trays made of ash, four feet long, two feet wide, and a few inches deep. Thermometer, Fahrenheit, 26 —: Product, Glauber salt, or sulphate of soda, and muriate of magnesia, in quantity corresponding with the proportion of the requisites in the mixture.

Observations.

Contrary to my usual practice, the saline liquor in this experiment was put out in a cold state: The consequence was, a precipitation of the sulphate of soda in the form and appearance of fl. benzoin. The whole of the Glauber salt was thrown down; the mother liquor containing only muriate of magnesia. While the mother liquor contains Glauber salt, it will feel and appear in some measure harsh and saline; but when totally deprived of Glauber salt, new appearances will take place. The tinned iron gutters, instead of shewing a disposition to rust, will be remarkably clean and brilliant; the mother liquor soft and oily to the touch, and will pour smooth. To obtain the Glauber salt in distinct crystals, the product of this experiment was

Very fine crystals of Glauber's salt.

redissolved in a leaden evaporator, in its own water of crystallization, and pure water added, enough to make a light saline liquor. This liquor was put into leaden coolers ten feet long, five feet wide, two inches deep, standing in a room, therm. 57, doors and windows were left open. In the morning I found and took about three or four large baskets of Glauber salts, perhaps the most perfect crystals ever seen; crystals from nine to fifteen inches long, and one inch broad.

Crystallization by frost without second solution.

To avoid the trouble and expence of dissolving the sulphate of soda twice, while the frost continued the operations were continued in the large way, in the proportions of sulphate magnesia, muriate soda, and water, as mentioned, with the precaution of pumping the liquor from the pans into the troughs and trays, not, but beneath the boiling point. The whole of the Glauber salt was always formed when the frost was severe, as at thermom. 23, in beautiful and distinct crystals, half an inch broad and four or five inches long.

PRECAUTIONS AND REMARKS.

Various cautions, &c. on the management of salts.

Cast iron pans, heated in frosty weather, frequently burst. Boilers should be made of sheet iron. Jointed wooden vessels will not hold hot saline liquors. In melting salts different steps must be adopted. Salt holding little water of crystallization,

as muriate of soda, should be added to a sufficient quantity of water; without a sufficient quantity of water in a heated pan it will fuse into a mass; surface being lost, the difficulty of solution is almost inconceivable. Various cautions, &c. on the management of salts.

Salts holding an abundance of water of crystallization, as sulphate of magnesia and sulphate of soda, should be dissolved by a gentle heat in their water of crystallization, and what farther water may be required, should be then added. Salts abounding in water of crystallization, added to warm water, will conglomerate, the caloric is absorbed, and although a small proportion of the salt is dissolved, the remainder is made colder: it is governed by the same law as operates on the solution of ice. The power of adding water evades a superabundant quantity of mother liquor, and ensures a quantity of crystals.

To obtain the saline liquor white and pellucid, it should not remain long in the iron pans, not more than two hours.

The first liquor pumped from the pans should be equally mixed with the last liquor, to obtain regular crystals in considerable quantity. It is known that the under layers of saline liquors are heaviest.

Very light liquors afford the largest shoots or crystals. Very large crystals appear opaque, and are never of a good colour; hence the necessity of breaking down to make many surfaces, to be acted upon by light.

Heavy saline liquors, holding Glauber salts, produce a quantity of salt in a compound state of crystallization.

A saline liquor, of a proper weight, will give numerous crystals, long and narrow, and these will be found in shape and colour most perfect. Such crystals were formed by the power of frost in the troughs and trays, acting upon *hot* saline liquor, in the *night*.

The *darkest* nights produced the best crystals. Perhaps the night was colder than the day. At any rate, the maxim of the necessity of light towards crystallization is doubtful in this instance. Light is not necessary to the crystallization of sulphate of soda. Light not necessary to the crystallization of salts.

The necessity of air towards crystallization, as shewn by the usual experiment of admitting air to a solution of salt in a vessel which did not previously contain air, is frequently exemplified in the large way. Salts sometimes will not shoot in a liquor covered Air is necessary.

covered by a strong pellicle. The workmen break the pellicle, and the salts instantly shoot. Crystallization, under such circumstances, is often *immediately excited* by gently running a saline liquor from one gutter into another gutter.

Electricity is of consequence.

Saline liquors, under an improper atmosphere, will not form regular crystals, but accrete into shapeless lumps. The electric and non electric states of the atmosphere govern crystallization. Many a maker of Glauber salts becomes fatigued from not understanding the latter circumstance.

Effect of frost in these experiments.

From what has been said, the young chemist must not suppose that the elective attractions are altered, and the new compounds in these experiments created by means of frost. From sulphate of magnesia and muriate of soda, sulphate of soda and muriate of magnesia can be obtained without frost. Frost only possesses a readier power of converting, from what was formed in a large portion of fluid, Glauber salt or sulphate of soda, speedily, from a hot liquid into a cold fluid state. The apparent cause consists in the power of severe frost in suddenly displacing the caloric, which becomes a disaggregative power.

H. CAMPBELL.

21, Fleet Street, May 20, 1802.

XIII.

Note of Citizen VAUQUELIN respecting the Boracite, called Magnesio-calcareous Borate by the French Chemists.

Component parts of the boracite.

THIS fossil, of which the properties have afforded a number of interesting observations to philosophers and mineralogists, has been analyzed for the first time by M. Westromb, who found it to contain,

Boracic acid	-	-	68
Magnesia	-	-	13,05
Lime	-	-	11
Alumine	-	-	1
Oxide of iron	-	-	0,75
Silex	-	-	2
			95,80

Citizen

Citizen Vauquelin having examined this substance some time ago with Mr. Smith, who brought a considerable quantity, thought himself justified in concluding, that the lime is no essential part in its composition, because its powder effervesces with the acids, and the small quantity of lime which chemists find in their analysis, did not appear to exceed that which the degree of effervescence has since indicated. They then attempted, by weak acids diluted with much water, particularly by the acetous acid, to separate the portion of carbonate mixed with the borate; but they did not succeed, because the acetous acid, even though feebly, likewise attacked the borate. They then left the question undecided, for want of transparent crystals which did not effervesce with the acids.

But since that period, Mr. Stromayer having supplied Citizen Vauquelin with crystals of this description, perfectly transparent, he subjected them to new experiments, with the intention merely of ascertaining the presence of lime.

He mixed their powder with muriatic acid, and when the solution was effected by means of a gentle heat, he evaporated to dryness in order to expel the excess of acid, and afterwards dissolved it in a small quantity of cold distilled water. By this method he separated most of the boracic acid, which was in very white brilliant plates. He diluted the solution with water, and mixed a certain quantity of oxalate of ammonia, which, as chemists know, is the best re-agent to shew the presence of the smallest quantity of lime contained in any fluid, provided it contain no excess of acid. Nevertheless, it exhibited no sign by which the existence of that substance could be suspected.

Experiments which shew that lime is not an essential part of this mineral.

In order to ascertain that the small quantity of boracic acid dissolved by water, at the same time as the muriate of magnesia, did not oppose the precipitation of the lime, he mixed a portion of the muriate of lime, which did not certainly amount to a fiftieth part of the borate employed, and a cloud was immediately produced through the whole of the fluid.

On the other part, he decomposed the artificial borate of lime in the same manner as the natural borate, and obtained, by the addition of oxalate of ammonia, a very abundant precipitate.

It is therefore evident, that if the natural borate had contained only one hundredth part of its weight of lime, it would have given some indication by the methods employed by C. Vauquelin.

quelin. He therefore concludes, that the natural magnesian borate, when perfectly transparent, does not contain lime, and that that which is found in opaque crystals is interposed in the state of carbonate, and causes their opacity.

This substance must not therefore any longer be considered as a triple salt, under the name of magnesian-calcareous borate, but simply that of magnesian borate.

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XIV.

*Facts and Observations tending to explain the curious Phenomenon of Ventriiloquism. By Mr. JOHN GOUGH *.*

The power of distinguishing the character of sound has been long considered; but not of its direction.

All sounds arrive in the ear from the same direction.

Our knowledge of the direction of sound does not depend on the impulse in the ear: but on other facts. Statement.

IN the excellent Paper from which I have extracted the following pages, the author begins by observing, that the power of the ear to distinguish very slight variations of tone has long been a subject of universal notice; but that another very remarkable power, namely that of ascertaining the direction of sound, remains still without explanation. We perceive not only the tone, intensity, and character of sound; but also whether it arrives from the right or the left, from above or below, and this with a degree of precision which is of great and eminent utility in the concerns of life.

To investigate the foundation of this habitual judgment, we cannot have recourse to analogy of the sense of hearing with that of vision. The last direction of the ray of light is physically impressed by the pencil which enters the organ of that sense: but in the ear the undulations of sound are all made to strike the instrument of perception in the same direction, namely that of the auditory passage. The author, therefore, is led to attend to other facts and observations. He found by experiment, with an instrument constructed to afford the same equal sound by blows from the action of a spring, that he could judge of an increase or diminution of distance, to the one hundred and twentieth part of the whole range. He also remarks,

* Partly abridged and partly extracted from his "Investigation of the Method whereby Men judge by the Ear of the position of Sonorous Bodies relative to their own Persons." *Manchester Memoirs*, vol. V. part 2, page 622. London, 1802.

that

that sound is most effective, when its course is most directly opposed towards the organ; and shews by experience, that the head itself is a sensitive solid, capable of perceiving sounds by their action upon its surface. Hence when the differences of effect of the same sound, from its difference of distance and direction with regard to the two ears, and the two sides or portions of the head are considered, there will be a sufficiently great variety in the sensations to mark the circumstance of direction, and produce an habitual power of discrimination in this respect as in all others, which continually affect our daily operations.

The annexed diagram, Fig. 2, Plate VII. is given to explain the phenomena which arise from the pulses of sound being obstructed by the hearer's head, as they move in the horizontal plane passing through his ears, which case ought to be treated separately from the more complex one that comprizes the angle of elevation, along with the horizontal distance from the axis of hearing. When the sonorous object stands directly in front of the hearer, the semicircle ACB may be supposed to represent the horizontal section of his head, passing through the places of the ears, E and F , and the axis of hearing EF ; also let G be the place of the sounding body, which, according to the conditions of the case, lies in the plane ACB produced, and likewise in the right line GS , which bisects EF at right angles; seeing then EF is bisected by the perpendicular SG , the arch ECF is also bisected by the same in the point C . Draw LG , GK , to touch the circle in T and P , then will the arcs TEC , and CFP be equal. Now all the pulses which do not move in right lines, contained in the angles TGS and SGP fly off without touching the circle; consequently they add nothing to the sound impressed on the ears by the body placed at G , whether the places E and F be supposed to lie in the arcs TC and CP , or without them. But the same number of pulses equal in force will fall in a given time, and in similar directions, on the arcs TEC and CFP as well as on the ears situated at E and F ; and it is equally manifest, that the same reasoning will apply to two equal and similar solids, constructed upon the equal and similar planes, ECS and FCS . Now sound, though it be formed in the ears, is very much increased by the vibrations excited in the contiguous parts of the head by the pulses

Diagram to shew
the effects of
sound on the
head.
Direct hearing.

pulses which fall upon them: therefore, as often as the two portions of the head, which are separated by the vertical plane perpendicular to the axis of hearing, are equally agitated by the pulses of the same sound, the ears are also equally affected from the same cause; which never happens, as we learn from the testimony of the other senses, unless the founding body be placed somewhere in the right line that bisects the axis of hearing at right angles. In this manner men are taught by experience to draw a general inference from a general observation; they therefore conclude a body to be situated directly before or behind their persons, as often as the sound of it strikes both their ears with equal forces.

Phenomena of oblique hearing.

The phenomena of oblique hearing remain to be explained; which case occurs as often as the founding body is situated in the horizontal plane, but not in the right line that bisects the axis of hearing at right angles. Let M be the place of the founding body, and draw MO to the centre of the circle; also let OC bisect the arc ECF , and take OG in it equal to OM : also draw WM , MR , PG , GI tangents to the circle. Now suppose a sound equal to that at M , to proceed from G , then the latter would have the same effect on the arc TCP that the former has on WDR , because the arcs are manifestly equal, and alike situated relative to the points M and G . But the sound proceeding from G is a case of direct hearing, consequently the ears placed at E and F receive equal impressions from it, which is not the case with the pulses that flow from M . For though the forces imparted to the two arcs, TCP , WDR are equal, they do not fall equally on the circle in respect to the points E and F , which represent the ears; the sound therefore coming from M strikes these organs with unequal forces, as may be easily inferred from the figure.

Practical delicacy of perception as to the course of sound.

Deception of hearing from echos.

The judgment of the direction of sounds by Mr. Gough's ear was precise to about eight degrees of position as to the horizon, and about ten degrees as to elevation above its plane.

The faculty by which men judge of the direction of sounds must inevitably be liable to deception, whenever the sonorous undulations are made to arrive in a direction which is not from the sonorous object, as in the case of echo; and these deceptions will be the more striking, as we are apt to rely on the testimony of the sense with the most implicit confidence.

Upon

Upon this which constitutes the supplementary part of the present essay, I shall copy my author without farther abridgement or selection, page 644.

" Any person who has had occasion to walk along a valley obstructed with buildings, at the time that a peal of bells was ringing in it, will assent to the truth of the circumstance here alluded to *. For the sound of the bells instead of arriving constantly, at the ears of a person so situated, in its true direction, is frequently reflected in a short time from two or three different places. These deceptions are in many cases so much diversified by the successive interpositions of fresh objects, that the steeple appears, in the hearer's judgment, to perform the part of an expert *ventriloquist* on a theatre, the extent of which is adapted to its own powers, and not to those of the human voice.

Instance of the sudden changes of direction of the sound of bells.

" The phenomenon has often attracted my attention ; and the similarity of effect which connects it with ventriloquism, convinces me every time I hear it, that what we know to be the cause in one instance is also the cause in the other : I mean that the echo reaches the ear, while the original sound is intercepted by *accident* in the case of the bells, but by *art* in the case of the ventriloquist. In order that the cause which gives rise to the amusing tricks of this uncommon talent may be pointed out with the greater clearness, it will be proper to describe certain circumstances that take place in the act of speaking, because the skill of the ventriloquist seems to consist in a peculiar management of them. Articulation is the art of modifying the sound of the larynx, by the assistance of the cavity of the mouth, the tongue, teeth, and lips. The different vibrations, which are excited by the joint operation of the several organs in action, pass along the bones and cartilages, from the parts in motion to the external teguments of the head, face, neck, and chest ; from which, a succession of similar vibrations is imparted to the contiguous air, thereby converting the superior moiety of the speaker's body into an extensive seat of sound, contrary to general opinion, which supposes the passage of the voice to be confined to the opening

This effect is similar to that of ventriloquism.

The human voice does not issue from the mouth only.

* Viz. that a sudden change of direction in sound will be perceived when the original communication is intercepted, provided there be a sensible echo.--N.

Instance.

of the lips: there are but few persons, I imagine, who have not some time or other witnessed an incident, which shews the vulgar notion to be erroneous in this particular. For if a man standing in a close apartment should happen to apply his face to a loop hole, or narrow window, in order to speak to some person in the open air, a by-stander in the room with him will hear his voice, not indeed in its natural tone, but as if it were smothered by being forced to issue from a hollow case; but the circumstance of his words being heard distinctly, by one who cannot receive them from his mouth, proves the vibrations requisite for their production to be conveyed through the solid parts of the speaker's body, agreeably to the preceding assertion. The reason why we generally conclude the voice to be confined to the opening of the mouth, appears to be this. Those pulses which escape from the aperture are the strongest, they therefore surpass the weaker vibrations of the contiguous parts; for when a number of sounds moving in different directions strikes the ear at the same instant, the hearer does not notice their several places, but refers all of them to the quarter in which the most powerful is perceived. For instance, when a man stands at a sufficient distance from an extensive obstacle, his words are answered by an echo; but let him make a loud uninterrupted noise, neither he nor any body near him hears two voices whilst his continues, but as soon as the noise ceases the echo is perceived. This does not happen because the one begins the moment the other ends; but the reflected sound being the weaker of the two, it is smothered by that which precedes it.

Effect of undistinguished echoes upon the voice or an instrument.

"We have seen in what manner secondary or reflected sounds are smothered by their principals; but though the places of such sounds are not recognized by the ear, their effects do not die away unnoticed: for the reverberated pulses mingle with those which come immediately from the sounding body, and thereby alter the sensation, which, without their interference, would be less compounded. This is the reason why the same musical instrument has one tone in a close chamber, where its notes undergo a multiplicity of reverberations, and another in the open air, where the reflections are few in comparison.

Case of an orator;

"But it is time to apply the preceding facts to the subject in hand; and it will be proper to begin with a familiar example. When an orator addresses an audience in a lofty and spacious room,

room, his voice is reflected from every point of the apartment, of which all present are made sensible by the confused noise that fills up every pause in his discourse; nevertheless every one knows the true place of the speaker, because his voice is the prevailing sound at the time. But were it possible to prevent his words from reaching any one of the audience directly, what then would follow? Undoubtedly a complete case of ^{who may become} ventriloquism would be the consequence, and the person so ^{a ventriloquist.} circumstanced would transport the orator, in his own mind, to the place of the principal echo, which would perform the part of the prevailing sound at the instant. This he would be obliged to do, because the human judgment is bound, by the dictates of experience, to regard the person as inseparable from the voice; and the deception in question would be unavoidable, being produced by the same concurrence of causes which makes a peal of bells, situated in a valley, seem to change place in the opinion of a traveller. It is the business ^{Conditions of} of a ventriloquist to amuse his admirers with tricks resembling ^{that art.} the foregoing delusion; and it will be readily granted, that he has a subtle sense, highly corrected by experience, to manage, on which account the judgment must be cheated as well as the ear. This can only be accomplished by making the pulses, constituting his words, strike the heads of his hearers, not in the right lines that join their persons and his. He must therefore know how to disguise the true direction of his voice, because the artifice will give him an opportunity to substitute almost any echo he chuses in the place of it. But the superior part of the human body has been already proved to form an extensive seat of sound, from every point of which the pulses are repelled, as if they diverged from a common centre. This is the reason why people, who speak in the usual way, cannot conceal the direction of their voices, which in reality fly off towards all points at the same instant. The ventrilo- ^{The sound must} quist therefore, by some means or other, acquires the difficult ^{issue from the} habit of contracting the field of sound within the compass of ^{mouth only,} his lips, which enables him to confine the real path of his voice to narrow limits. For he, who is master of the art, has nothing to do but to place his mouth obliquely to the company; and to dart his words, if I may use the expression, against an opposing object, whence they will be reflected immediately, so as to strike the ears of the audience from an ^{and it must be} unexpected ^{conveyed by echo.}

unexpected quarter, in consequence of which the reflector will appear to be the speaker. Nature seems to fix no bounds to this kind of deception, only care must be taken not to let the path of the direct pulses pass too near the head of the person who is to be played upon; for, if a line joining the exhibitor's mouth and the reflecting body approach one of his ears too nearly, the divergency of the pulses will make him perceive the voice itself instead of the reverberated sound.

Narrative of a
ventriloquist.

"The only ventriloquist I ever attended, acted in strict conformity to the preceding theory of this curious paradox in the science of acoustics. His audience was arranged in two opposite lines, corresponding to the two sides of a long narrow room. The benches on which they were seated reached from one end of the place to the middle of it, the other part remaining unoccupied. The feats exhibited by him were the

Voice of a child
from beneath
certain benches:

three following. *First*: he made his voice come from behind his audience, but it never seemed to proceed from any part of the wall, near the heads of the people present; on the contrary, it was always heard resembling the voice of a child, who seemed to be under the benches. He stood during the time of speaking in a stooping posture, having his mouth turned towards the place from which the sound issued; so that the line joining his lips and the reflecting object, did not approach the ears of the company. *Second*: advancing into the vacant part of the room, and turning his back to the audience, he made a variety of noises, that seemed to proceed from an open cupboard which stood directly before him, at the distance

noises from a
cupboard,

cries from an in-
verted cup.

of two or three yards. *Third*: he placed an inverted glass cup on the hands of his hearers, and then imitated the cries of a child confined in it. His method of doing it was this; the upper part of the hearer's arm laid close along his side; then the part below the elbow was kept in an horizontal position with the hand turned downwards, which was done by the operator himself. After taking these preparatory steps, the man bent his body forwards in a situation which presented the profile of his face nearly to the front of his hearer, whilst his mouth pointed to the cup; in which posture he copied the voice of a confined child so completely, that three positions of the glass were easily distinguished by as many different tones, *viz.* when he pressed the mouth of the cup close against the palm, when one edge of it was elevated, and

Method of ope-
rating.

when

when the vessel was held near the hand but did not touch it. The second and third instances of ventriloquism afford strong proofs, that this delusive talent is nothing more than the art of substituting an echo for the primary sound; for, besides the change perceivable in the direction of the voice, it was found to be blended with a variety of secondary sounds; such as we know by experience are produced as often as a noise of any kind issues from a cavity. I have already made some remarks on this species of knowledge; but it would be improper to dismiss the subject without noticing the accuracy, with which the ear recognizes the finer modifications of sounds, and their causes. I have frequently observed, that a certain waterfall makes a flatter and duller noise when the ground is covered with snow, than that which it affords at other seasons. The human voice also undergoes a similar change within doors, by striking a multiplicity of soft bodies, such as a number of piles of wool, or a crowded congregation in a church.

Remarks and observations.

The method of preventing the vibration of the vocal organs from reaching the external teguments, is still wanting to complete this theory of ventriloquism; and I presume it can only be supplied by an adept in the art. I must therefore dismiss the subject unfinished, because I have no pretension to that character.

Difficulty not solved.

XV.

*An Analysis of a Mineral Substance from North America, containing a Metal hitherto unknown. By CHARLES HATCHETT, Esq. **

IN the course of the last summer, when I was examining and arranging some minerals in the British Museum, I observed a small specimen of a dark-coloured heavy substance, which attracted my attention, on account of some resemblance which it had with the Siberian chromate of iron, on which at that time I was making experiments.

Specimen of mineral observed in the British Museum.

Upon referring to Sir Hans Sloane's catalogue, I found that this specimen was only described as "a very heavy black stone,"

Historical particulars.

Philos. Transf. 1802.

VOL II.—JUNE, 1802.

K

"with

"with golden streaks," which proved to be yellow mica; and it appeared, that it had been sent, with various specimens of iron ores, to Sir Hans Sloane, by Mr. Winthrop, of Massachusetts. The name of the mine, or place where it was found, is also noted in the catalogue; the writing however is scarcely legible: it appears to be an Indian name, (Nautneauge) but I am informed by several American gentlemen, that many of the Indian names (by which certain small districts, hills, &c. were forty or fifty years ago distinguished,) are now totally forgotten, and European names have been adopted in the room of them. This may have been the case in the present instance; but, as the other specimens sent by Mr. Winthrop were from the mines of Massachusetts, there is every reason to believe that the mineral substance in question came from one of them, although it may not now be easy to identify the particular mine.

§ I. DESCRIPTION OF THE ORE.

Description.

The external colour is dark brownish gray.

The internal colour is the same, inclining to iron gray.

The longitudinal fracture is imperfectly lamellated; and the cross fracture shews a fine grain.

The lustre is vitreous, slightly inclining in some parts to metallic lustre.

It is moderately hard, and is very brittle.

The colour of the streak or powder is dark chocolate brown.

The particles are not attracted by the magnet.

The specific gravity, at temp. 65°, is 5918*.

Experiment 1.

The ore was digested in muriatic acid.
Little action.

Some of the ore, reduced to fine powder, was digested in boiling muriatic acid for about one hour.

* The following results of some experiments which I have purposely made, will shew how much the specific gravity of this ore is different from that of Wolfram, and Siberian chromate of iron.

Pure Wolfram, free from extraneous substances, at tem. 65° 6355.

Siberian chromate of iron, containing some of the green oxide 3728.

Pure Siberian chromate of iron - - - 4355.

The Siberian chromate of iron, like all other mineral substances which are not crystallized, and which consequently are not always homogeneous, must evidently be liable to considerable variations in specific gravity.

The

The acid appeared to have acted but slightly upon the powder; as the former remained colourless, and the latter did not seem to be diminished. A portion, however, chiefly of iron, was found to be dissolved; for ammonia formed a yellow flocculent precipitate; prussiate of potash produced one which was blue; and tincture of galls, when the excess of acid had been previously saturated by an alkali, formed a precipitate of a rich purplish brown colour.

Experiment 2.

Another portion of the powder was, in like manner, digested with nitric acid; but, excepting some slight traces of iron, this acid afforded nothing worthy of notice; the action of it upon the ore, was indeed scarcely perceptible. In nitric acid: little action.

Experiment 3.

Some of the pulverized ore was digested with concentrated sulphuric acid, in a strongly-heated sand-bath, until nearly the whole of the acid was evaporated; the edges of the mass then appeared blueish, and became white, when boiling distilled water was added. In sulphuric acid, Slight action.

This acid certainly acted much more powerfully than those which have been mentioned; but still only a small part of the ore was dissolved. It must however be observed, that a very copious blue precipitate was obtained by prussiate of potash; a plentiful purplish brown precipitate was also produced by tincture of galls, after the excess of acid had been saturated by an alkali; and, lastly, when the yellow ferruginous precipitate formed by ammonia was dissolved in diluted nitric acid, some white flocculi remained, which were completely insoluble in the acid, even when it was added so as to be in considerable excess.

From these experiments it was evident, that the ore could not readily be decomposed by the direct application of the mineral acids; and I therefore had recourse to the following method, which has frequently been employed with success in similar cases.

ANALYSIS.

A.

A mixture of 200 grains of the powdered ore with five times the weight of carbonate of potash, was exposed to a strong red heat. Fusion with carbonate of potash.

K 2

heat,

heat, in a silver crucible. As soon as the matter began to flow, a very perceptible effervescence took place; and, when this had subsided, the whole was poured into a proper vessel.

The mass, when cold, was grayish-brown.

Solution in water left a brown residue.

Boiling distilled water was poured upon it; and the brown residuum, which was considerable, was well edulcorated upon a filter.

Precip. by nitrous acid: white flakes.

The filtrated liquor had a slight yellowish tinge, and, being supersaturated with nitric acid, afforded a copious white flocculent precipitate, which speedily subsided; but, although a very considerable additional quantity of nitric acid was poured upon the precipitate, it was not re-dissolved.

The residue did not yield to fusion by potash.

The residuum of the ore was dark brown, and was again melted with potash, and treated as before; but scarcely any effect was thus produced; the alkali was therefore washed off, and the powder was digested with muriatic acid, which soon assumed the deep yellow colour usually communicated to it by iron. After half an hour, the acid was decanted, and the residuum was washed with distilled water.

It was therefore digested with mur. acid, which took up iron.

This residue was fused with potash, and precipitated as before.

This powder was now of a much paler colour; and, being mixed with potash, it was melted and treated as before. A considerable precipitate was again obtained by the addition of nitric acid; and the residuum, after being digested with muriatic acid, was again fused with potash, by which means the whole was completely decomposed, after about five repetitions of each operation.

What was left was treated in the same manner.

B.

The muriatic solution saturated with ammonia afforded the iron.

The muriatic solution was diluted, and, being saturated with ammonia, afforded a plentiful ochraceous precipitate; which again was dissolved in cold dilute nitric acid, and afforded a small quantity of a white insoluble substance, similar to that which was obtained from the alkaline solution. From this nitric solution, I then obtained, by means of ammonia, a precipitate of oxide of iron, which, being properly dried, weighed 40 grains.

C.

The nitric precipitates were a white matter.

The different alkaline solutions which had been made subsequent to that which has been first mentioned, were mixed together, and, being supersaturated with nitric acid, afforded the same white insoluble precipitate; the total quantity of which, obtained from 200 grains of the ore, amounted to about 155 grains.

The

The liquor from which this precipitate had been separated and the fluid by nitric acid, was then saturated with ammonia, and, being gave a little more iron. boiled, afforded about two grains of oxide of iron.

I obtained, therefore, from 200 grains of the ore,

	Grains.	Grains.
Oxide of iron	42	} = 197.
And of the white precipitated substance	155	

But, as I could not repeat the analysis without destroying the remaining part of the only specimen at present known of this ore, I do not wish the above stated proportions to be regarded as rigidly exact; it will be sufficient, therefore, to say at present, that the ore is composed of about three parts of the white matter, and rather less than one of iron.

§ II. PROPERTIES OF THE WHITE PRECIPITATE.

A.

It is of a pure white, and is not extremely heavy.

The white precipitate

It has scarcely any perceptible flavour, nor does it appear to be soluble in boiling water; when, however, some of the powder is placed upon litmus paper moistened with distilled water, the paper in a few minutes evidently becomes red.

B.

1. When examined by the blow-pipe, it is not fusible *per se* in a spoon of platina, nor upon charcoal, but only becomes of a less brilliant white.

2. Borax does not appear to act upon it; for the white particles are only dispersed throughout the globule. not affected by borax.

3. It produces an effervescence when fused with carbonate of soda, and forms a colourless salt; but, if too much of it be added, then the mass, when cold, appears like a white opaque enamel. Soluble by fusion with soda.

4. When carbonate of potash is employed, the effects are similar in every respect to those of soda; and it may here be remarked, that the saline combinations thus formed with soda, or potash, are soluble in water; and that these solutions have the same properties as that which was formed when the ore was decomposed by an alkali. The portion of the white precipitate which may be in excess, subsides unaltered, when the globules are dissolved in water. and with potash, as in the first decomposition.

Phosphate of ammonia forms a deep blue globule.

5. Phosphate of ammonia produces a very marked effect; for, when melted in a platina spoon, if some of the white substance be added, a considerable effervescence takes place, and the two substances rapidly unite. The globule, when cold, is deep blue, with a tinge of purple, but, when held between the eye and the light, it appears of a greenish gray colour.

C.

It resists nitric acid:

It is perfectly insoluble, and remains unchanged in colour, and in every other respect, when digested in boiling concentrated nitric acid.

D.

is soluble in sulphuric acid: which by dilution lets fall a sulphate,

It is dissolved by boiling sulphuric acid, and forms a transparent colourless solution, which is however only permanent while the acid remains in a concentrated state; for, if a large quantity of water be added to the solution, or if the latter be poured into a vessel of distilled water, the whole in a few minutes assumes a milky appearance, and a white precipitate is gradually deposited, which cracks as it becomes dry upon the filter, and, from white, changes to a lavender blue colour, and again, when completely dry, to a brownish gray. It is then insoluble in water, has not any flavour, is semi-transparent, and breaks with a glossy vitreous fracture.

This substance is much heavier than the original white precipitate; and in a very slight degree may be dissolved by boiling muriatic acid, or by boiling lixivium of potash.

Upon examining these solutions, I found that both contained the original white substance, together with some sulphuric acid; so that the precipitate obtained from the sulphuric solution by the addition of water, is a sulphate of the white matter*.

The whole is not however precipitated by water; for a part remains in solution, which may be separated from the sulphuric acid by either of the fixed alkalis, or by ammonia.

The sulphuric solution afforded an olive precipitate by prussiate of potash;

The sulphuric solution is not rendered turbid by the addition of water, until some minutes at least have elapsed; when,

* This sulphate is also precipitated when the sulphuric solution has been long exposed in an open vessel to the air; and, according as this may be moist or dry, the effect is produced sooner or later.

therefore,

therefore, some prussiate of potash was added immediately after the water, the colour of the liquor became olive green, and a copious precipitate, of a beautiful olive colour, was gradually deposited.

Tincture of galls, after a few minutes, caused the liquor to become turbid, and a very high orange-coloured precipitate was obtained.

A few drops of phosphoric acid were added to a part of the concentrated sulphuric solution; and, after about 12 hours, the whole became a white opaque stiff jelly, which was insoluble in water.

Potash, soda, and ammonia, whether pure or in the state of carbonates, separate the substance in question from the sulphuric solution, in the form of a white flocculent precipitate; and, when these alkalis are added to a considerable excess, they do not redissolve the precipitate, unless they are heated; then, indeed, the fixed alkalis act upon it, and form combinations which have already been mentioned, but which we shall soon have occasion more particularly to notice.

E.

1. The white precipitate, when recently separated from potash, is soluble in boiling muriatic acid; and this solution may be considerably diluted with water, without any change being produced.

2. A part was evaporated to dryness, and left a pale yellow substance, which was not soluble in water, and was dissolved with great difficulty, when it was again digested with muriatic acid.

3. Prussiate of potash changed the colour of the muriatic solution to an olive-green; the liquor then gradually became turbid, and an olive-coloured precipitate was obtained, similar to that which has been lately mentioned. But,

4. If some nitric acid was previously added to the muriatic solution, then the prussiate changed the liquor to a grass-green, but did not produce any precipitate.

5. Tincture of galls, in a few minutes, formed an orange-coloured precipitate, like that which has been mentioned; but, if the acid was in too great an excess, it was necessary to add a small quantity of lixivium of potash or soda, before the precipitate could be obtained.

White flakes by
phos. acid,

6. A small quantity of phosphoric acid, being added to the muriatic solution, in a few hours formed a white flocculent precipitate.

and by alkalis.

7. Potash, soda, and ammonia, also produced white flocculent precipitates, which were not redissolved by an excess of the alkalis, unless the liquors were heated; and, in that case, part was dissolved by the fixed alkalis, but not by ammonia.

Muriates of
lime, magnesia,
and strontian,
gave no precip.

8. The muriatic solution did not yield any precipitate, when the muriates of lime, magnesia, and strontian, were added; but muriate of barytes formed a slight cloud.

Zinc threw
down white
flakes.

9. When a piece of zinc was immersed in the muriatic solution, a white flocculent precipitate was obtained *.

F.

Acetous acid
does not dissolve
the white mat-
ter.

The acetous acid has not any apparent effect on the white precipitate, when long digested with it.

G.

Fixed alkalis
readily combine
with it.

The fixed alkalis readily combine with this substance, both in the dry and in the humid way.

We have already seen, that the former method was employed with success in the analysis of the ore; and the experiments made with the blow-pipe may be regarded as an additional confirmation. In each of these cases, the white precipitate combined with the alkali, as soon as the heat was sufficient to cause the latter to flow; and, when a carbonate was employed, a portion of carbonic acid was expelled.

The carbonic acid was in like manner disengaged, when the white precipitate was boiled with lixivium of carbonate of potash, or of soda; and the solutions thus prepared, resembled in every respect those which were formed by dissolving in water the salts which had been produced in the dry way.

It will be proper here to give a more particular account of these combinations.

Humid solution
in potash: a
portion left,

1. Some of the white precipitate was digested, during nearly one hour, with boiling lixivium of pure or caustic potash:

* This appears to indicate the obstinacy with which this substance retains a certain portion of oxygen; for we here see that zinc does not precipitate it in the metallic state, but only reduces it to an insoluble oxide.

about

about one-fourth of the powder was dissolved; and the remainder, which appeared little if at all altered, subsided to the bottom of the vessel.

The clear solution, which contained a great excess of alkali, affords a scaly white precipitate; and, by gentle evaporation, yielded a white glittering salt; in scales, very much resembling the concrete boracic acid.

The salt was placed upon a filter, so that the lixivium might be separated. It was then washed with a small quantity of cold distilled water; and, being dried, remained as above described, although constantly exposed to the open air.

This salt had an acrid disagreeable flavour, and contained a small excess of alkali. It did not dissolve very readily in cold water; but, when dissolved, the solution was perfect and permanent.

Some nitric acid was added to part of the solution, and immediately rendered it white and turbid. In a short time, a white precipitate was collected, similar to that which had been employed to neutralise the potash: and the clear supernatant liquor, being evaporated, only afforded nitre.

Prussiate of potash was added to another portion; but did not produce any effect, until some muriatic acid was dropped into the liquor, which then immediately assumed a tinge of olive green, and slowly deposited a precipitate of the same colour.

Tincture of galls did not affect the solution at first; but, when a few drops of muriatic acid had been added, it gradually lost its transparency, and yielded an orange-coloured precipitate.

2. As so large a part of the white precipitate had remained undissolved in the foregoing experiment, it was digested again with another portion of the same lixivium, but without any effect. I therefore washed off the alkali, and boiled some nitric acid with the powder, until the acid was completely evaporated. After this, the powder was exposed to a strong heat in a sand-bath. It was then again digested with the lixivium, and a part was dissolved as before; but still the residuum required to be treated with nitric acid, before the alkaline liquor could again act upon it; so that it was necessary to repeat these alternate operations several times, before the whole of the powder could be united with the alkali.

The portion not dissolved in potash—was not soluble till after treatment with nitric acid.

It left a residue having the same properties.

3. When

Carbonate of soda, or potash, acted very nearly as pure potash.

Precip. by the tungstate and molybdate of potash, and cobaltate of am. Hydro-sulph. am. gave reddish precip. The white matter dissolved in alkali, and iron dissolved in alkali, gave, by mixture, a precipitate of the original ore.

3. When the white precipitate was digested with solution of carbonate of potash, or of soda, it was dissolved, much in the same manner as above related; and the properties of the solutions, when examined by reagents, were also similar, excepting that the orange-coloured precipitates produced by tincture of galls were of a paler colour.

Tungstate of potash, molybdate of potash, and cobaltate of ammonia, being severally added to the solution of the white substance in potash, produced white flocculent precipitates.

Hydro-sulphuret of ammonia produced a reddish chocolate-coloured precipitate.

4. As the ore was decomposed by being fused with potash, the following experiment affords a curious instance (among the many already known) of the change in the order of affinities produced by a difference of temperature.

Some of the solution of the white precipitate in potash, was poured into the alkaline solution of iron, which was formerly known by the name of Stahl's *Tinctura Alkalina Martis*. Potash was in excess in both of these solutions; but nevertheless a cloud was immediately produced, and a brown ferruginous precipitate was deposited.

Part of this precipitate was dissolved in muriatic acid; and the solution, being examined in the usual way, yielded a blue precipitate when prussiate of potash was added, and a purplish brown precipitate with tincture of galls.

The other part of the precipitate was digested with dilute nitric acid; which dissolved the ferruginous part, but left untouched a white flocculent matter, perfectly resembling the substance which has been so often mentioned. The precipitate therefore produced by the mixture of the two alkaline solutions, was a combination of the white matter with oxide of iron, very similar to the original ore.

(To be continued.) /

XVI.

Remarks on the Mamoth. By LOUIS VALENTINE, Physician in Chief of the Army and Hospitals of America, of several National and Foreign Societies, resident in Nancy.

Bones of a large extinct animal found in America.

SINCE settlements have increased in North America, a great quantity of bones have been found belonging to some extinct animal, which seems to have resembled an elephant, but

was much larger. By digging near the lakes of Canada, where the animal is called by the savages, the Father of Oxen; near the rivers which fall into the Ohio; towards the rivers Miami, Muskingum, in the state of Kentucky, and of Tennessee, &c. &c. but principally near the salt springs, pieces of skeletons and tusks have been found, of an astonishing length and weight.

We have seen a femur and a tibia, which, when united, must have been five feet and a half high; another femur, which was alone five feet long, and thirty-six inches in circumference in its middle or cylindrical part; ivory tusks resembling those of an elephant, which were near seven feet long, and one foot six or eight inches in circumference at the base. It was not till the year 1800 that a complete skeleton of these fossil bones could be procured. Two physicians of Philadelphia, Doctors Barton and Wistar, had in their possession the lower jaw almost entire, with two teeth on either side, in particular, that of the former has five and three points, all quite double; but no one had the entire head.

Enormous magnitude by ad-measurement.

The state of New York (in the environs of the beautiful river Hudson) has of late years been the theatre of discoveries of fossil bones, a greater quantity of them having been found there than any where else. In 1800, by digging in the low and marshy places of the counties of Orange and Ulster, at three, four, and five feet deep, parts, which had never been before discovered, were found. Some bones, ten feet deep in the earth, were as found and entire as those which had been met with nearer the surface. Some, however, were found broken, particularly those of the head.

Where most plentifully found.

In another place, eight miles from the city of New York, an upper jaw was found perforated to receive a tusk like that of an elephant; the connection of the tusks was by *gomphosis*; the tusks were evidently of ivory; the openings for the nostrils were eight inches in diameter; and notwithstanding that the bones of the feet afford reason to conclude that the animal had claws, it is scarcely possible to avoid thinking, from the structure of the head, that it was a species of elephant. Some hair has even been found, three inches in length and of a dark colour, which is said to have belonged to this monstrous quadruped. Doctor Graham, a Senator, in a letter to Doctor Mitchell, Representative in Congress, and Professor of chemistry

Particular facts.

Conjectures as to the species of animal.

and

Question as to
final causes.

and natural history at the college of Columbia, says on this occasion, that it is difficult to resolve the question; "Why Providence should have destroyed this species, which it was pleased to create?" Yet if it was voracious, it is happy for the human species that it has by any means become extinct.

The animal has
never been seen
within human
memory or tra-
dition.

We may reasonably doubt the assertion of those authors who affirm, that certain savages, Russians and Greenlanders, have seen this animal living, and that it still exists in the north. All that has been transmitted by tradition to the oldest Indians who have communication with the United States, and from whom information has been sought concerning this object, is quite fabulous, and does not offer even a shadow of probability. Mr. Jefferson, now President of the United States, formerly paid great attention to this subject. (See his Account of Virginia.)

Conjectural re-
marks.

It is certain that in Siberia and in Greenland, where similar fossil bones have been found, no one has brought proof of having seen this animal living. We cannot suppose it to be cetaceous or amphibious, of the nature of the hippopotamus, as there are sufficient reasons to prove the contrary. Mr. Cuvier, whose researches into the extinct species of animals are no less curious than scientific, distinguishes the animal of Siberia, which affords fossil ivory, from the mammoth, which latter differs principally from the former on account of its magnitude, and the points of its grinders, &c.

An entire skele-
ton,

Since the time of our having received details on this subject (see the New York Medical Repository, Vol. IV.) and what we have related elsewhere (see the third edition of Guthrie's Geography, in French, Vol. VI, page 225 and 262, published by Langlois at Paris), an account has been published in the American papers in 1801, that Mr. William Peal, proprietor of the museum at Philadelphia, having collected the bones found in the county of Orange, in the state of New York, had succeeded in forming a skeleton, the height of which is twelve feet; the head being four feet and a half in length, and the tusks ten feet; the other parts being in the same proportion.

twelve feet high,
and its head four
feet and a half
long.

Local situation
of these enor-
mous bones.

Almost all these bones have been found in calcareous earth. The bones of the megalonix, or great claw, of which Mr. Jefferson has given a description, were found in caverns of lime stone and chalk in Tennessee. The other enormous bones of the megatherium, found in such great quantities in the county of Ulster, of which Sylvanus Miller, Esq. has given some detail

detail in a letter to Professor Mitchill, are found in strata of marl, which are dug to procure this calcareous substance as a manure for land. It is, nevertheless, sometimes remarked, that these bones begin to fall in small portions when, after having been from their calcareous inclosure, they are exposed to the atmosphere. Teeth, which were sound and entire when extracted from the earth, become black in a short time, crack, lose their enamel, and fall into small scales. Without such a preservative we may presume, with our friend Mitchill, that the remains of these animals would have been decomposed many centuries ago.

They sometimes
perish by expo-
sure to the air.

SCIENTIFIC NEWS, &c.

Prizes of the National Institute of France.

THE class of mathematical and physical sciences having proposed in the year 8, as the subject of a prize to be awarded at the public sitting of the 15 Germinal, in the year 10, the following question: *What are the characters which distinguish in vegetable and animal matters, those which serve as ferments, from such other bodies as they put into a state of fermentation?* And the memoirs which have been received not answering the conditions of the program, the class proposes the same subject again for the year 12. The prize is of the value of one kilogramme (about one hundred and twenty-five pounds sterling). It will be given at the public meeting of the 15 Germinal, in the year 12. Memoirs must be sent before the 1st Nivose of the same year.

Prize medal for
a memoir on fer-
ments and fer-
mentable bodies

Astronomical Prize.—Citizen Lalande has presented the National Institute of France with the sum of ten thousand francs (about 400 guineas) to found an annual prize, to be given by the Institute to the author of such discovery, observation, or work in astronomy, as shall be thought the most remarkable or useful, during the course of the year. The Institute very highly applauded this act of generosity in one of its members, and decreed, that thanks should be assigned in their registers, and commissaries nominated by each of the three classes, to present at the next general meeting the means of execution.

Astronomical
prize.

Communications to the Royal Society respecting the Planet Ceres.

Dr. Herschel sent an account of the appearance of the new planet, as viewed through his telescopes. He had sought for it

Extreme small-
ness of the planet
Ceres.

it in vain, until he received Dr. Maskelyne's determination of its place. When viewed with powers of 600 and 1200, it could not be decidedly distinguished from a star, until it was found to change its place. Its apparent diameter was not large enough to be directly determined, but it was certainly not larger than one-fourth of that of the Georgian planet, and perhaps equal only to one-sixth. From a rough computation of its magnitude, Dr. Herschel concludes that its real diameter is about $\frac{1}{3}$ of that of the moon: its light is of a reddish hue.

Mr. Gilpin also gave the Society an account of observations on the 8th and 12th of February. He found the planet's right ascension change from $188^{\circ} 41'$ to $188^{\circ} 30'$, while its declination increased. Mr. Gilpin observes that its light resembles that of the planet Mars.

Nebulous atmosphere.

Thursday, 25th February. A letter from Mr. Schroeter of Lilienthal, respecting the planet Ceres Ferdinandia, informed the Society that Mr. Schroeter had observed a nebulosity round the planet, somewhat resembling that of a comet: the diameter of the true disc being $1.8''$, and that of the nebula $2.6''$, but the distinction was not always equally observable. Mr. Schroeter considers this body as of a hybrid nature, or a medium between a planet and a comet; but he imagines the apparent nebulosity to be owing to an atmosphere, and that, according to the different states of this atmosphere, the light reflected from the planet is either white, bluish, or reddish.

A table of observations of the same planet was also communicated by Mr. Mechain, through Sir Henry Englefield.—*(This article is taken from the Journal of the Royal Institution.)*

Leonardo da Vinci.

The lovers of the polite arts will be pleased to learn, a new translation of Leonardo da Vinci's treatise on painting (for which Poussin made the figures) will soon be ready for publication. This work has been long in the hands of Mr. Rigaud, R. A. who has paid it particular attention and care; and has given new importance and energy to the work, by arranging the chapters successively under proper heads; by which the student will be much facilitated in understanding the precepts of this great master in the art of painting.

The reader will recollect some particulars of the extraordinary researches and very superior genius of Da Vinci, by the extracts given in the former series of this Journal, from an abridgement of his writings, by J. B. Venturi, quarto, II. 84.

Experiments

Experiments to prove that all Bodies, whatever may be their Nature, are obedient to the Action of Magnetism, and that this Action is sufficiently powerful to admit of being measured. By Citizen COULOMB.*

It has long ago been remarked, that platina, nickel, and several other bodies, acquire a sensible degree of magnetism; but some philosophers attribute this property only to a portion of iron not easy to be separated, and conclude, that by obtaining a greater degree of purity, we might succeed in rendering them perfectly indifferent to the action of the magnetic bar.

Magnetism of nickel, &c. supposed to be accidental.

The new experiments which Citizen Coulomb has made and repeated before the Institute, lead us on the contrary to think, that the action of magnetism extends through all nature; for none of the bodies he has yet tried was found to resist this power.

All bodies are magnetical.

But however real this action may be, it is not alike in all bodies, and in most of them it must be necessarily very small, to have escaped the attention of philosophers to this time. In order, therefore, to exhibit and to measure these results, we must begin by placing the bodies in a situation which shall allow them to yield to the weakest action.

For this purpose, Citizen Coulomb fashioned his subjects into the form of a cylinder or small bar, and in this state he suspended them to a filken thread, such as is drawn from the silk worms' cone, and in this state he placed them between the opposite poles of two magnetic bars of steel. The single thread of silk could hardly bear the weight of a quarter of an ounce without breaking, consequently it became necessary to form small bars very light and thin. Citizen Coulomb made them about seven or eight millemetres in length (or less than half an inch), with three quarters of a millimetre (or about an hundredth part of an inch) in thickness, and he gave the metals about one-third of this thickness.

Experiments made with bodies suspended to a single fibre of silk.

In his experiments he placed the steel bars in the same right line, their opposite poles being five or six millemetres farther asunder than the length of the needle intended to oscillate between them. The result of the experiment shewed, that whatever might be the substance of the needle, it always disposed

All bodies assumed the direction of two magnets which were applied.

* Communicated to the French National Institute, and inserted in the *Magazin Encyclopedique*, No. 22, an 10.

itself according to the direction of the two bars; and that if they were turned from this direction, they always recovered it, after oscillations of which the number was often more than thirty per minute. It was therefore easy in every case to determine, from the weight and figure of the needle, the force which had produced the oscillation.

Subjects of experiment.

These experiments were successively made with small needles of gold, silver, copper, lead tin, small cylinders of glass, a piece of chalk, a fragment of bone, and different kinds of wood.

Extreme delicacy of the suspension.

Citizen Coulomb has proved, in a former memoir, that the force of torsion of the silk thread is so slight, that in order to draw it round through the entire circle, it would require a force scarcely equal to the one hundred thousandth part of a gram (or about one seven hundredth of a grain). A quantity so minute cannot therefore sensibly derange the measure of magnetic force in the different bodies, and its effect even, if it were admitted to be of perceptible magnitude, may also be urged in proof of the general conclusion of Citizen Coulomb; because the magnetic power must overcome this resistance of the thread in order to manifest itself. Our author gives, in the third vol. of the Memoirs of Natural Philosophy and Mathematics of the National Institute, a very simple formula to determine the magnetic force of a body from the time of its oscillations, and he means to shew in another memoir, the method of determining this result in different bodies of the same figure placed between the poles of two bars. He thinks it now proved, that all the elements which enter into the composition of our globe, are subjected to the magnetic power, and that the whole mass collectively forms one single magnet.

Observations.

In favour of those who might be desirous of repeating his experiments, and rendering them very sensible, the author remarks, that the method of succeeding consists in diminishing the size of the oscillating bodies. From some essays, of which the results terminate this memoir, it seems to follow, that the accelerating forces are inversely as the masses are very nearly in the direct proportion of the surfaces; but Citizen Coulomb gives this rule only as a first deduction, which requires to be confirmed.

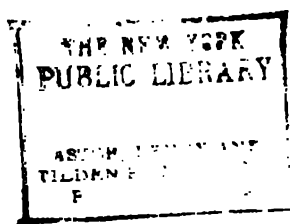
ERRATA.

In Mr. Cruickshank's paper, p. 43, end of the first paragraph, for *faults* read *fasts*; and p. 46, l. 26, for *computed* read *compelled*.

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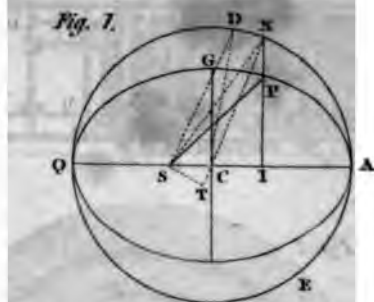
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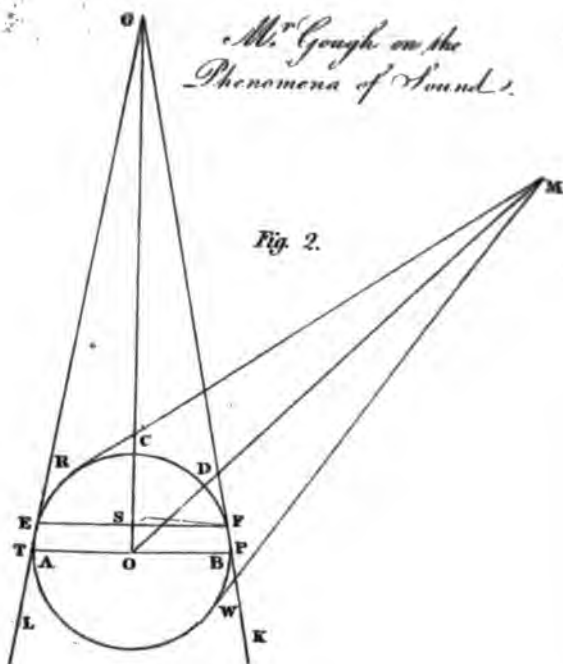
*Mr Gregory's Investigation of
Mr. Pearsons's Analogy. &c.*

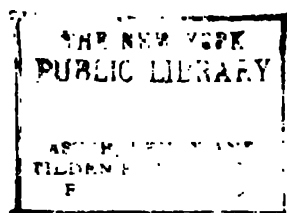
Fig. 1.



*Mr. Gough on the
Phenomena of Sound.*

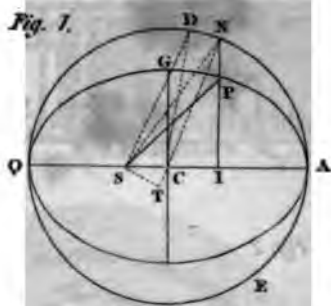
Fig. 2.





*Mr. Gregory's Investigation of
Mr. Pearsons Analogy &c.*

Fig. 1.



*Mr. Gough on the
Phenomena of Sound.*

Fig. 2.

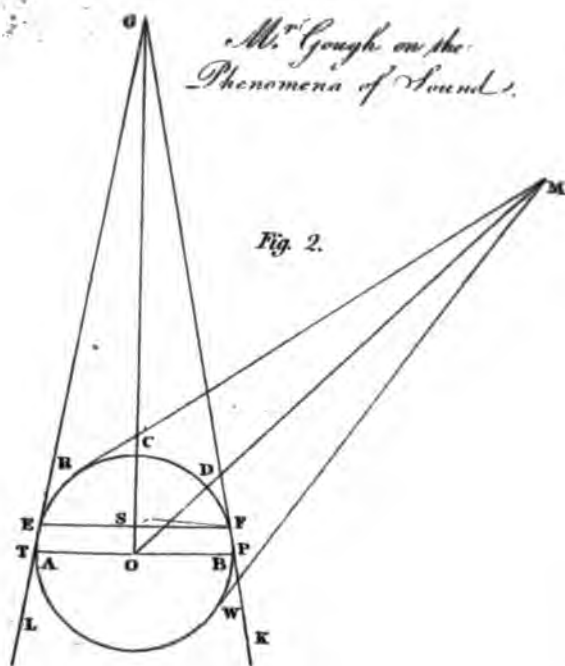


Fig. 5.

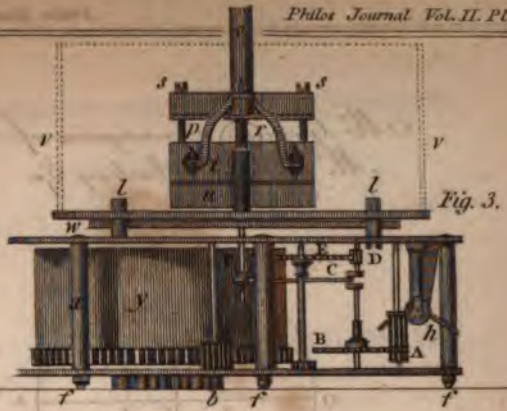


Fig. 3.

Fig. 2.

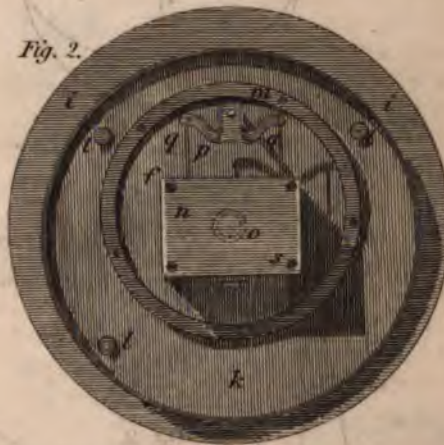


Fig. 6.



Fig. 4.



Mechanical Lamp of Carvel & Carreau.

Fig. 1.



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A
JOURNAL
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NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JULY, 1802.

ARTICLE I.

Composition of Writing Ink, possessing the permanent Colour, and other essential Properties, of the Ink used for Printing. In Letter from Mr. WILLIAM CLOSE.

To Mr. NICHOLSON.

S I R,

Dalton, May 25, 1802.

IN the second volume of the Philosophical Journal, at p. 63, Preliminary address.
the preparation of indelible ink is announced. Such an article may justly be considered a desideratum. As I have for some time directed my attention to the composition of permanent ink, I send you a short memoir on the subject, which was drawn up previous to the perusal of the above-mentioned notice, and intended for a future communication.

THE welfare of individuals often depends very much on Permanent ink, the testimony of writing. An atramentous composition, possessing a permanent colour on paper, and such a degree of an important article for the preservation of authentic testimony in writing, infolubility, after desiccation, as not to suffer any injury from exposure to humidity, or the application of such chemical preparations

VOL II.—JULY, 1802.

L

parations

parations as totally eradicate the traces of common ink, would certainly be an article of considerable value in the material composition of those writings, or legal instruments, which are intended to evince the transactions of the day, and to ratify the affairs of the future.

The invention of printing probably produced some remissness about the durability of ink.

The ancients were probably more interested in the composition of permanently coloured ink than the moderns. When every book was in manuscript, it would frequently be requisite to transcribe whole volumes, sometimes merely for the purpose of procuring a copy, at other times for the preservation of the work itself; in either case, before the writer begun a process so tedious, it would be of consequence to select such materials as were the most durable, in order to protract the like necessity, and also for the purpose of enhancing the value of his labour. But since the invention of printing, the necessity of transcribing has been entirely superseded; the manuscripts of modern authors after publication, have been reduced to little more value than objects of curiosity; and writers have been contented with such materials as are the cheapest and easiest to procure.

Ink with vegetable infusions and sulphate of iron,

not durable.

The colour may be restored while the sulphate of iron remains on the paper. The most serious objection to its use is, that it may be totally discharged.

Means of preventing this by the addition of pigments.

For a great number of years, the ink most generally used by European writers has been the infusion of galls, and other astringent vegetables containing gallic acid, rendered black by sulphate of iron, and thickened by the addition of a little gum or fugar. This composition is well adapted to the common purposes of writing and the dispatch of business, but its colour is liable to fade: a composition of a more permanent colour is therefore desirable, for writings which are required to retain their primitive signatures, and such as cannot be printed. Indeed, since the discovery of the method of totally discharging the traces of common ink by the application of the oxygenated muriatic acid, more serious consequences are to be apprehended from the universal use of the common atramentous fluid, than the decay of its colour from age; for it is well known, that while the sulphate of iron remains on the paper, the colour of the writing may be restored, by washing the manuscript with fresh infusion of galls.

In the quarto series of the Philosophical Journal (Vol. IV. p. 479), there are several useful receipts for composing ink capable of resisting the oxygenated muriatic acid. The most material difference, however, of the compositions there re-

commended from those in common use, consists in the addition of pigments producing an unchangeable colour upon paper.

Ink of a permanent colour may be easily obtained, by suspending various pigments in an aqueous fluid, by the intervention of gum, without the assistance of the common ingredients; but such compositions are liable to one of the greatest inconveniences, for the whole of the writing may be detached from the paper by washing the manuscript with water. Such ink, however, was frequently used by the ancients.

Ancient method of composing ink.

As a permanent colour is certainly a valuable requisite, it appears very promising in speculation for the improvement of this indispensable article, according to the simplest, and perhaps the most ancient method of composition, to substitute, in place of the common mucilaginous fluid, as a compound vehicle for the distribution and protection of the colouring matter, the solution of some gum, or resinous substance, which can be dissolved in only a few liquids. After the dissipation of the thinner part of an atramentous compound properly formed with such a solution, the colouring substance will be left on the paper, combined with a sufficient quantity of tenacious matter to protect it from being injured by friction, or from being discharged by the application of any fluid to which the writing may be exposed, without injuring the paper.

Speculation for the improvement of this kind of ink.

Many of the more volatile kinds of oils may be used in writing, if reduced to a proper consistence by the addition of gum or resin. Tolerable ink may be made by dissolving 30 grains of common resin in 90 grains of oil of turpentine, and tempering the solution with $17\frac{1}{2}$ grains of lamp black, and $2\frac{1}{2}$ of indigo. In a dry state, this composition resists the action of water, but not of spirit. Such, indeed, will be the case with every composition in which the colour is merely suspended in the fluid, and attached to the paper, by a substance of easy solubility; the application of the article which produced the fluidity of the ink, will again penetrate and soften the dry compound. Those compositions which contain a tenacious matter, soluble in a few articles only, and at an high temperature, will be the least exceptionable.

Ink with oil and resin.

Inconvenience to which every composition will be subject when the colour is mechanically suspended.

A more insoluble kind of matter than resin should be used. Copal will dissolve in only a few liquids; it appears well adapted to the purpose of retaining a permanent colour upon

Copal is much superior to resin for the suspension of the pigment;

the paper, if a vehicle can be found which will dissolve a sufficient quantity, and which, when the colouring matter is also added, shall be fluent enough in writing.

but is not altogether free from inconvenience.

Oil of lavender will dissolve copal. I have made some experiments with these articles, and the combination succeeds so well, that I have not been inclined to use any other. The only inconvenience to be apprehended from the use of copal in the composition of ink, is, from its being soluble at a low temperature; but whether any other kind of tenacious matter of more difficult solubility, can be used conveniently in writing, I must leave to future discussion. The solution of a substance which could only be dissolved in an high temperature, would certainly be the most proper vehicle to prevent alteration; for, after the desiccation of the writing, it would be difficult to soften the ink in one part of a manuscript, without exposing the whole to a very injurious process.

Composition of ink with oil of lavender, copal, and lamp black.

Ink may be composed of oil of lavender, copal, and lamp black, according to the following proportions of the ingredients:

Take of oil of lavender, 200 grs. copal in powder, 25 grs. lamp black from $2\frac{1}{2}$ to 3 grs. With the assistance of a gentle heat, dissolve the copal in the oil of lavender in a small glass phial, and then mix the lamp black with the solution upon a marble slab, or other smooth surface. Put the composition into the bottle, and keep it excluded from the air. After a repose of some hours, the ink must be well shaken and stirred with a piece of wire before it is used; if it be too thick, it must be diluted with a little oil of lavender, oil of turpentine, or alcohol. The facility of writing with this composition depends much on the quantity of the colouring matter. Three grains of lamp black to two-hundred and twenty-five of the solution of copal, producing ink of a full body of colour, and is nearly as much as the copal can protect from injury after the dissipation of the oil of lavender. Two grains and an half of lamp black, and half a grain of indigo, produces ink of a paler colour, but which may be distributed upon the paper with the facility of common ink. A piece of sponge*, or other organized

* The custom of putting cotton in an inkstand has been disused, more particularly, I think, since an inkstand, invented by the late worthy and ingenious Samuel More, of the Society of Arts, was made

ganized substance, must be used in the inkstand, chiefly for the purpose of cleaning the pen.

Red ink may be made by tempering the solution of copal with red sulphuret of mercury instead of lamp black. The following proportions of the ingredients produce a red ink which writes very well:

Take of oil of lavender 120 grs. copal in powder, 17 grs. red sulphuret of mercury, 60 grs. Dissolve the copal in the oil of lavender, and then mix the sulphuret with the solution upon a smooth surface.

Both these compositions possess a permanent colour, and other essential properties of the ink used in printing. The oil of lavender being dissipated with a gentle heat, the colour is left on the paper surrounded with copal, a substance insoluble in water, in spirits, in acids, or alkaline solutions. A manuscript, written with these compositions, may therefore be exposed to the process commonly used for restoring the colour of printed books, without the smallest injury to the writing; and in this manner all interpolations with common ink may be removed.

Red ink with red sulphuret of mercury.
A manuscript written with these compositions will not be injured by the process of bleaching.

As

made and brought into fashion about thirty years ago, by the celebrated Wedgwood. These changes appear to be both injurious. For the ink in the cotton is kept blacker by the suspension of the atramentous part; and if no more ink be present than perfectly to fill the cotton, the pen will always receive a fluid black ink, and may be charged at pleasure by a greater or less gentle pressure at the time of taking up, or discharged by lodging the point for a moment upon the cotton. It is also very easy to regulate the oxygenation by the air, so as to increase the blackness without suffering mouldiness to come on, by the simple expedient of turning the cotton upside down every day. As the fibres of the cotton prevent the fluid from circulating as usual by the change of temperature produced from evaporation, the interior mass may be considered as in a closed vessel while not in use.

Mr. More's fountain inkstand, at present so universally in use, is certainly very inconvenient. The ink, it is true, is kept in a closed vessel; but its colouring matter is at full liberty to subside, and the consumer is obliged to fill his pen from the muddy bottom instead of the surface; and what is still worse, the conical vessel into which the ink flows, is subject to all the evils of evaporation and mouldiness, so as most frequently to afford an adhesive and clogging fluid to the pen. W. N.

Source of apprehension. The oil of lavender will soften the dry composition, but the writing cannot be easily eradicated.

Amber a proper substance if it could be used.

As copal is soluble in several of the essential oils, it may be expected that the application of these will do much injury to the writing. Such an expectation is well founded. If the writing be rubbed with a smooth surface dipped in oil of lavender, much of the colouring matter will be disengaged, and distributed more diffusely upon the paper; but, as some of it will have penetrated the interior parts of the paper along with the copal, it will be extremely difficult to obliterate the traces of the pen without the erasure being perceptible. This is the principal source of apprehension, but I know of no method of obviating the danger. The perusal of a memoir on the nature and preparation of drying oils, &c. by Mr. Sheldrake (*Philosophical Journal*, 8vo. Vol. I. p. 259), suggests amber as a proper article for the composition of ink, if enough of it can be dissolved in any fluid sufficiently thin for the purpose of writing. The more difficulty there is in effecting the solution of any tenacious substance we use, the better, provided it can then be employed without inconvenience.

I am, Sir,

Yours respectfully,

WILLIAM CLOSE.

II.

Experiments and Observations on the Vegetation of Plants, which shew that the common Opinion of the Amelioration of the Atmosphere, by Vegetation in Solar Light, is ill founded. By JAMES WOODHOUSE, M. D. Professor of Chemistry in the University of Pennsylvania, &c.

To Mr. NICHOLSON.

SIR,

Pater Noster Row, May 27, 1802.

Introductory letter.

I ENCLOSE for the *Philosophical Journal*, the results of various experiments, made in Philadelphia in the year 1801, upon the seeds, leaves, &c. of a variety of plants, which seem to prove, that growing vegetables, contrary to an opinion almost universally adopted, do not purify atmospherical air; and that
whenever

whenever they appear to afford oxygenous gas, it is by devouring the coal of carbonic acid gas for food, and leaving its oxygen in the form of pure air.

I have the honour to be,

Dear Sir,

With the greatest respect,

Your most obedient,

And very humble servant,

JAMES WOODHOUSE.

First. Of the Effects produced by the Germination of Seeds in atmospherical Air.

On the 3d of June, twelve seeds of zea maiz were planted in earth, and confined over water in a glass vessel, in seventy ounce measures of atmospherical air of the purity of 100, and often exposed to the light of the sun. On the 12th, the corn had vegetated, and was from two to five inches high. The air being examined at this time, by throwing up one measure of it, over lime water, in an eudiometer, gave $\frac{73}{100}$ parts of carbonic acid air. Another measure, after being freed from the fixed air, and mixed with an equal measure of nitrous air, produced an absorption of $\frac{30}{100}$. On the 19th, the corn having grown considerably, and the air being tried again, no carbonic acid gas appeared, and the purity was the same as at first. On the 23d, the plants died, and the air was found to consist of $\frac{5}{100}$ fixed, and $\frac{5}{100}$ azotic gas.

Germination of seeds in atmospherical air.

Similar experiments were made with the seeds of apium petroselinum, lactuca sativa, cucurbita citrullus, phaseolus sativus, sisymbrium, and raphanus sativus, and with the same result.

The atmospherical air, in these experiments, appears to be reduced in purity, by its oxygen uniting to the coal of the cotyledons of the seed, or to that of some animal or vegetable matter contained in the earth in which the seeds are planted, or to that of some decayed portion of the living leaves.

The air lost oxygen by uniting to carbon; afterwards it became more pure; and lastly its oxygen was totally absorbed.

Ingenhouz, Humboldt, and Thomson, have observed, that soils have the property of absorbing oxygen; but as it cannot be proved that any pure earth, or mixture of earths, render atmospherical air impure, it is certainly more philosophical to ascribe the impurity of the air to the formation of the carbonic acid, the base of which generally exists in all soils.

The effect of veg. mould on air most probably from its carbon.

II, *Of the Effects produced by the Growth of Plants in atmospheric Air.*

Growth of plants
in atmosf. air.

On the 27th of May, twelve plants of *periscaria polygonum*, two inches high, growing in earth, were confined in a glass vessel in fifty-two ounce measures of atmospheric air, of the purity of 100, and often exposed to the influence of solar light. On the 4th of June, they had increased about two inches in height. The air being examined at this time, was found to contain $\frac{1}{100}$ parts of carbonic acid gas, and to be reduced in purity to 80. Several young plants of *rhapanus fativus*, *lactuca stramonium*, *phytolacca decandra*, *zea maiz*, *phaseolus fativus*, *sidum telephium*, *amaranthus hyboidus*, *cucurbita citrullus*, *grymbrium*, and *lactuca sativa*, were also separately confined in from forty to eighty ounce measures of atmospheric air, which was examined at various times, from one hour to thirty days, after the plants had been placed in it. Carbonic acid gas was generally formed, and whenever this circumstance happened, the purity of the air was diminished.

They produced
carbonic acid
gas, and dimi-
nished the purity
of the air.

Growth of plants
in oxygen dimi-
nished its purity by
carbonic acid.

Many of the same kind of vegetables were also confined in forty ounce measures of oxygenous gas, which had been well washed in lime water, and the purity of the air was very generally lessened, fixed air being generated. They turned of a white or yellow colour, and soon died, after being placed in atmospheric air.

In confined
plants the de-
caying parts af-
ford carbon and
form acid, which
the living plant
decomposes.

The same effects are produced by the growth of plants as by the germination of seeds in common air, and by the same causes. If the leaves are confined a considerable time, part of them decay, and the coal of the dead portion, uniting with the oxygen of the atmospheric air, generates carbonic acid. This acid is decomposed by the living leaf. Its coal is abstracted, while its oxygen is left in the form of pure air.

But when the
formation is
quicker than the
decomposition,
the plant dies.

When the oxygen unites to the coal of the animal or vegetable matter of the soil in which the plants vegetate, or to the coal of the decayed parts of the leaves, and makes fixed air quicker than the living parts can decompose it, the plants will speedily die.

When the soil
contains but
little of organ-
ized remains,
the included
plant will live
much longer.

When a plant in perfect health, growing in a soil which contains little vegetable or animal matter, is confined in atmospheric air, it will live a long time, without producing any change in it. Many of the vegetables which were the subjects of these experiments,

experiments, did not affect the air in five days: some diminished its purity in three hours; and others altered it in a most slow and gradual manner, causing little change in it in twenty days.

III. *Of the Effects produced by the Leaves of Plants in atmospheric Air impregnated with Carbonic Acid Gas, and exposed to the Light of the Sun.*

A handful of the leaves of *mimosa virgata*, *euphorbia pifta*, *digitalis purpurea*, *franklinia altamaha*, *asparagus officinalis*, *coryllus avellana*, *rhus glabrum*, *aristotichia fiphee*, and *periploca græca*, were separately expofed feven hours to the light of the fun, in thirty-fix ounce meafures of atmofpheric air, impregnated with four ounce meafures of carbonic acid gas, from the carbonate of lime and fulphuric acid. The fixed air difappeared, and the atmofpheric air was fo much increafed in purity, as to devour two meafures of nitrous air.

The leaves of these plants, kept over night in the same air, gave carbonic acid gas in the morning; and its purity, in every instance, was considerably diminished.

The leaves of *mimosa virgata* and *amygdalus perfica*, were also separately exposed nine hours to the influence of solar light, in forty ounce measures of atmospheric air, in which fixed air had been formed by leaving a fungus to putrefy it. The carbonic acid gas disappeared, and the purity of the atmospheric air was increased from 50 to 80.

IV. The following Tables will shew the Quantity and Purity of oxygenous Gas, obtained by exposing a small Handful of the Leaves of Plants to the Light of the Sun, in forty Ounce Measures of Pump Water.

This water was taken from a well sunk within a few yards of a necessary, from which it was impregnated with carbonic acid gas, as appeared from an analysis. The leaves were separately expofed in glaffes arranged near each other, and from eight to thirteen comparative experiments were made at one time.

Leaves of	Carbonic Acid Gas, in 100 Parts.	Oxygenous Gas in Oxygen Measure.	Boiling with pure Mixture of Nitrous Air.	Do. with two Mea- sures.	Do. with three Measures.	Base of the Ther- mometer.	Time when exposed.
<i>Alcea rosea</i> - - -	19	122	146	96	103° to 110° of Fahrenheit.		July 2, 1802. The day was very clear.
<i>Zea maiz</i> - - -	16	116	140	54			
<i>Amaranthus spinosa</i> -	15	120	140	68			
<i>Melissa officinalis</i> - -	13	120	130	50			
<i>Hyssopus</i> - - -	16	120	138	70			
<i>Convolvulus purpureus</i> -	8	110	110	0			
<i>Malva rotundifolia</i> - -	17	120	140	86			
<i>Lavendula</i> - - -	16	118	130	55			
<i>Rosa centifolia</i> - - -	15	112	130	46			
<i>Mirabilis dichroma</i> - -	16	110	130	40			
<i>Convolvulus purpureus</i> -	13	110	120	40	100° to 115°.		July 3. Day clear.
<i>Anthemis nobilis</i> - - -	12	114	120	32			
<i>Hibiscus Syriacus</i> - - -	12	118	130	65			
<i>Polygonum aviculare</i> - -	18	114	130	50			
<i>Amygdalus Perica</i> - - -	10	114	112	12			
<i>Pyrus malus</i> - - -	16	116	120	20			
<i>Platanus occidentalis</i> -	12	120	140	20			
<i>Tilia Americana</i> - - -	10	120	138	40			

Leaves

Leaves of	Carbonic Acid Gas in 100 Parts.	Oxygenous Gas in Drachm Measures.	Purity with one Measure of Nitrous Air.	Pos. with two Measures.	Pos. with three Measures.	State of the Thermometer.	Time when exposed.
<i>Siriodendron tulipifolia</i>	-	14	112	120	25	103° to 110°.	July 4, 1801.
<i>Populus dilatata</i>	-	14	110	132	60		Day generally clear.
<i>Æsculus pavia</i>	-	13	110	130	60		
<i>Apium petroselinum</i>	-	12	115	132	55		
<i>Convolvulus purpureus</i>	-	5	120	120	30		
<i>Helianthus annuus</i>	-	13	112	132	62		
<i>Ruta graveolens</i>	-	10	120	130	40		
<i>Trifolium palustre</i>	-	13	120	140	55		
<i>Datura stramonium</i>	-	14	112	130	80		
<i>Hyopos</i>	-	7	112	132	65		
<i>Blattaria verbascum</i>	-	12	112	130	45	90° to 110°.	July 5.
<i>Chelidonium majus</i>	-	18	112	136	80		Day clear and cloudy.
<i>Chrysanthemum Indicum</i>	-	14	120	142	63		Twelve ounce measures of this oxygenous air, after being washed in lime water, to free it from the carbonic acid gas, being exposed to a mixture of iron filings and sulphur, were found to consist of eight ounce measures of oxygenous, and four of azotic gas.
<i>Acer glaucum</i>	-	14	120	139	63		
<i>Phytolacca decandra</i>	-	14	120	140	80		
<i>Antirrhinum linaria</i>	-	18	120	140	65		
<i>Arctum cappa</i>	-	12	120	140	53		
<i>Syringa vulgaris</i>	-	8	120	132	40		
<i>Helianthus altissimus</i>	-	12	120	140	55		
<i>Polygonum Persicaria</i>	-	12	120	140	80		
<i>Cercis Canadensis</i>	-	12	120	140	60		
<i>Sonicera caprifolium</i>	-	12	120	140	60		
<i>Diospyros Virginiana</i>	-	10	120	120	30	90° to 110°.	July 6.
<i>Franklinia altamaha</i>	-	10	120	102	0		Day clear and cloudy.
<i>Chionanthus Virginica</i>	-	8	120	100	0		
<i>Arundo gigantia</i>	-	10	120	130	32		
<i>Asclepias Syriaca</i>	-	9	120	80	0		
<i>Annona triloba</i>	-	10	120	130	40		
<i>Magnolia glauca</i>	-	10	110	102	0		
<i>tripetala</i>	-	16	116	130	40		
<i>Xanthoriza tinctoria</i>	-	8	120	130	50		
<i>Conferva cicularis</i>	-	10	120	120	30		
<i>Alcea rosea</i>	-	5	110	70	0		
<i>Sophora indica</i>	-	7	110	80	0	90° to 110°.	These leaves were gathered in the evening, and kept until morning, in a cool place.
<i>Laurus sassafras</i>	-	10	120	92	0		

Opinion that
plants supply ox-
igen to the at-
mosphere,

ill founded;

because they af-
ford none unless
carbonic acid be
present. Expe-
riments of
Priestley in
proof;

We are indebted to Dr. Priestley for the discovery, that plants exposed to light yield oxygenous air; and ever since it has been made, an opinion has been adopted, that growing vegetables supply the oxygenous portion of atmospherical air, of which there is a constant consumption, by combustion, fermentation, respiration, and the calcination of metals.

If this subject is attentively examined, it will be found that plants have no effect in rendering the air of the atmosphere pure.

First. Whenever oxygenous gas has been obtained from vegetables, carbonic acid gas has been present.

Dr. Priestley exposed plants to atmospheric air, in which spirit of wine and wax and tallow candles had burned out; to air which had been vitiated by the death or putrefaction of mice and fishes, and to air which had been frequently taken into his lungs. He also observed, that there was a slower and less production of air from rain and distilled, than from pump and stagnant water.

and of the au-
thor; tabulated.

The difference between the quantity and quality of the gas, obtained from river water and the same water impregnated with carbonic acid, by exposing plants in it to the influence of solar light, will be seen by the following table:

Leaves of	Carbonic Acid Gas, in 100 Parts.	Quantity of Gas in Drachm Measures.	Purity with one Measure of Nitrogenous Air.	Do. with two Measures.	Do. with three Measures.	State of the Thermometer.	Time when exposed.
Siriodendron tulipifera -	None.	From half a Drachm to one Drachm.	55			110°.	July 7, 1801.
Cercis Canadensis -			70				Day very clear.
Tilia Americana -			50				The leaves were exposed in the water of the river Schuyltrill.
Salix Babylonica -			32				
Polygonum Perficaria -			30				
Phytolacca decandra -			94				
Platanus occidentalis -			90				
Alcea rosea -			84				
Helianthus annuus -			83				
Amygdalus Perfica -			82				
Conserva fontinalis -			80				
Zea maiz -			75				
Acer glaucum -			90				
Siriodendra tulipifera -	In some of the vessels none, in others, from 5 to 10 Parts.	6 120	130	40		110°.	July 8, 1801.
Cercis Canadensis -		6 116	124	30			Day a little hazy, although the sun shone constantly.
Tilia Americana -		5 110	160	0			The leaves of the same plants, in the same river water, impregnated with four quarts of the water, saturated with carbonic acid gas, from carbonate of lime and the sulphuric acid.
Salix Babylonica -		5 120	100	0			
Polygonum Perficaria -		10 120	140	70			
Phytolacca decandra -		6 120	140	42			
Platanus occidentalis -		3 110	60	0			
Alcea rosea -		6 120	132	40			
Helianthus annuus -		10 120	110	50			
Amygdalus Perfica -		6 120	138	40			
Conserva fontinalis -		4 120	134	50			
Zea maiz -		4 115	125	20			
Acer glaucum -		6 120	140	50			

It appears from this table, that the leaves of thirteen different plants, separately exposed in forty ounce measures of the water of the river Schuyltrill, produced about ten drachm measures of air, the principal part of which was azotic gas; whereas the same kind of leaves, exposed in the same quantity of the same water, impregnated with carbonic acid, yielded seventy-seven drachm measures of oxygenous air, of a very high degree of purity.

Count

Count Rumford's experiments to obtain oxygen from water by solar light.

Remarks on Priestley's

and Count Rumford's experiments.

Direct experiments

with fibrous bodies, which gave air less in quantity and purity than leaves.

Other sources of air.

Count Rumford made an attempt, in the year 1787, to overthrow the doctrine of the purification of the air by plants. His arguments were, that leaves confined in water were in unnatural circumstances, and that pure air could be obtained from other bodies, as fine spun glass, raw silk, common cotton, and that of the poplar tree, exposed in water to the light of the sun *.

The ingenious author of *Phytologia* also says, it may be suspected that, in many of the experiments of Priestley and Ingenhouz, the production of vital air might be simply owing to the action of the sun's light on the water in which the vegetables were immersed, like that from the silk in the experiments of Count Rumford; and that the fine points or sharp edges of these bodies, contributed only to facilitate the liberation of it when exposed to the sun shine, which thus disoxygenated the water by their united effect.

The experiments of Count Rumford are far from being satisfactory. Thirty grains of raw silk, at the end of three days, yielded him but $3\frac{1}{2}$ cubic inches of air, and sometimes four days elapsed before a sufficient quantity could be collected for an experiment.

In order to find how much air could be obtained from the "fine points or sharp edges" of certain bodies acting upon water, the following substances were exposed one day to the action of solar light, in forty ounce measures of pump water.

Filaments of asbestos, baked horse-hair, common cotton, and that of the *asclepias Syriaca*, the flower panicles of *rhus cotinus*, the fine hairy plumes of *climatis crispa*, the spikes of *panicum glaucum*, and charcoal in powder. From each of these substances, from two to four drachm measures of pure air were obtained, which devoured nearly two measures of nitrous air; consequently it was less pure than that procured from leaves exposed in the same water. There was also a much smaller quantity of it; for from eight to nineteen drachm measures may be obtained in a few hours, by immersing the leaves of any plant in the same water, and exposing them to solar light.

Some water, without any mixture, will yield oxygenous gas by the combined action of light and heat; and many substances placed in water, appear to act merely by raising its temperature.

* Transactions of the Royal Society for 1787.

The green vegetable matter, which forms on all bodies, immersed a considerable time in water, might also have been one of the sources of pure air, in some of the experiments of Count Rumford.

Secondly. Many philosophers suppose, that vegetables yield oxygenous gas by the decomposition of water. Its hydrogen is said to enter into plants, while its oxygen is set at liberty in the form of pure air.

Plants do not decompose water; for they do not operate in pure water.

If this opinion was true, oxygenous gas should be obtained by exposing leaves in boiled, rain, distilled, river, or lime water, but this cannot be done.

Thirdly. Some suppose that vegetables give oxygenous air to animals, and that the latter yield them azotic gas in return, which they devour for food.

Plants do not (as has been supposed) emit oxygen and absorb azote;

If this hypothesis were just, atmospheric air would be increased in purity by confining leaves in it when it contained no fixed air; and its purity might also be increased, after being previously diminished, by an additional quantity of azotic air, in the same manner.

A handful of the leaves of *euphorbia picta*, *nicotiana tobacco*, *buxus vulgaris*, *cinna glauca*, *mimosa julibrescens*, *jaxus procumbens*, *coryllus avellana*, *Herculea foetida*, *malva crispa*, *pinus strobus*, *colutea arborescens*, and *epilobium*, were separately exposed four hours to the light of the sun, in forty ounce measures of atmospheric air, and its purity was found to be neither increased nor diminished. After they had remained sixteen hours in the air, no effect was produced on it. The leaves were fresh gathered, and no decay could be observed upon any part of them.

for fresh leaves do not affect atmospheric air,

When leaves are plucked promiscuously, and are placed in atmospheric air either in the day or night, they diminish its purity. Wherever a leaf is perforated, and this is very generally done by insects, let the perforation be ever so small, the part decays, and the coal of this decayed part uniting to the oxygen of the atmospheric air, generates carbonic acid, which lessens its purity.

though wounded or decaying leaves do.

Experiments in
solar light ;

The following table shews the effect of the leaves of plants gathered promiscuously, exposed five hours to the light of the sun, in forty ounce-measures of atmospherical air, at a temperature of 75° of Fahrenheit.

	Fixed Air.	Atmospheric Air of the fixed and Acid of azote.
A small handful of the		
Leaves of <i>Datura stramonium</i> - -	3	96
<i>Rhododendron maximum</i> - -	5	87
<i>Apium petroselinum</i> - -	4	86
<i>Anthemis nobilis</i> - -	0	100
<i>Sophora australis</i> - -	2	95
<i>Sedum telephium</i> - -	0	100
<i>Amaranthus hybridus</i> - -	10	70

in the dark.

The following table will shew the effects produced in one night, on forty ounce measures of atmospheric air of the purity of 100, by a small handful of leaves gathered promiscuously from a variety of plants.

	Fixed Air.	Atmos. Air.
Leaves of <i>Ilex aquifolium</i> - -	5	88
<i>Juniperus officinalis</i> - -	4	93
<i>Berberis vulgaris</i> - -	2	86
<i>Franklinia alatamaha</i> - -	3	85
<i>Rhododendron maximum</i> - -	1	95
<i>Annona triloba</i> - -	2	88
<i>Buxus vulgaris</i> - -	2	90
<i>Pinus strobus</i> - -	2	88
<i>Mitchella repens</i> - -	0	100
<i>Arctostaphylos</i> - -	5	86
<i>Hamamelis Virginica</i> - -	0	100
<i>Bignonia radicans</i> - -	3	77
<i>Xanthoxylum tinctoria</i> - -	1	94
<i>Magnolia tripetala</i> - -	5	67
<i>Kalmia latifolia</i> - -	2	85
<i>Pinus picea</i> - -	3	80
<i>Siriodendron tulipifera</i> - -	10	65

According to some philosophers, carbonic acid gas is secreted by certain vegetables in the night; but as the quantity of this air obtained is always in proportion to the decayed parts of plants,

plants, and to the temperature to which they are subjected, it appears more rational to ascribe the generation of it to the coal of the decayed parts uniting with the oxygen of the air in which they are placed.

To determine whether plants would absorb or devour azotic gas, eight ounce measures of this air were mixed with thirty-two ounce measures of atmospheric air, so that its purity was reduced from 100 to 91. A handful of the leaves of *euphorbia picata* and *corylus avellana* were separately confined in forty ounce measures of this air, and exposed to the influence of a bright solar light five hours. No carbonic acid gas was generated, and the purity of the air was exactly the same as when first tried. No decayed portion could be observed upon these leaves.

As it is acknowledged that the leaves, stems, and roots of plants, separate the oxygen from carbonic acid, it may be said, that the oxygenous portion of atmospheric air is supplied by the decomposition of this gas, as it is always found in the atmosphere, and often in water in which vegetables grow.

The quantity of carbonic acid gas in atmospheric air, is reckoned to be about one part in an hundred. It must, however, vary in different places. We would expect to find the most of it in cities, where it is formed by combustion, respiration, fermentation, and putrefaction. If one measure of the air of any large city is thrown up over lime water in an eudiometer, no milky appearance will be produced, so that the quantity of carbonic acid in this air must be extremely small. As this gas is also seized upon by alkalis, earths, and metals, and absorbed by water, the quantity floating in the atmosphere may be less than one part in ten thousand.

When we consider likewise, that the oxygen is never separated from the carbonic acid by leaves, but when they are exposed, in contact with it, to the light of the sun; and that every perforation made in a living leaf, however minute, by an insect, causes the part to decay, and absorb oxygen by day and by night; and that, in the autumn in some countries, all leaves fall on the ground, ferment or putrify, and thus diminish the purity of common air; and that the petals and fruit of vegetables have the same effect, we must pronounce, that the oxygenous portion of atmospheric air cannot be supplied by vegetation.

Leaves exposed to sunshine in a mixture of atmospheric air and azote, produced no effect.

Leaves do not purify the atmosphere by decomposing its carbonic gas; because the quantity of this gas is very minute.

and plants deteriorate the air much more.

Air bladders of various plants contain air worse than that of the atmosphere.

Dr. Darwin supposes, that the air in the air bladders of vegetables serve to oxygenate the feed. The air of the air bladders of *cardiospermum halicacabum*, *staphylia trifoliata*, *colutea arborefcens*, and *sophora australis* being examined, was found to be a little worse than the air of the atmosphere.

III.

On the Theory of Light and Colours. By THOMAS YOUNG, M. D. F.R. S. *Professor of Natural Philosophy in the Royal Institution.*

(Concluded from page 90.)

PROPOSITION IV.

Prop. IV. Partial reflection of undulations at the confine of mediums differing in density.

When an Undulation arrives at a Surface which is the Limit of Mediums of different Densities, a partial Reflection takes place, proportionate in Force to the Difference of the Densities.

THIS may be illustrated, if not demonstrated, by the analogy of elastic bodies of different sizes. "If a smaller elastic body strikes against a larger one, it is well known that the smaller is reflected more or less powerfully, according to the difference of their magnitudes: thus, there is always a reflection when the rays of light pass from a rarer to a denser stratum of ether; and frequently an echo when a sound strikes against a cloud. A greater body striking a smaller one, propels it, without losing all its motion: thus, the particles of a denser stratum of ether, do not impart the whole of their motion to a rarer, but, in their effort to proceed, they are recalled by the attraction of the refracting substance with equal force; and thus a reflection is always secondarily produced, when the rays of light pass from a denser to a rarer stratum." (Phil. Transf. for 1800, p. 127.) But it is not absolutely necessary to suppose an attraction in the latter case, since the effort to proceed would be propagated backwards without it, and the undulation would be reversed, a rarefaction returning in place of a condensation; and this will perhaps be found most consistent with the phenomena.

PROPOSI-

PROPOSITION V.

When an Undulation is transmitted through a Surface terminating different Mediums, it proceeds in such a Direction, that the Sines of the Angles of Incidence and Refraction are in the constant Ratio of the Velocity of Propagation in the two Mediums.

Prop. V. Law of refraction of undulations transmitted through the confine of different mediums.

(Barrow, Lect. Opt. II. p. 4. Huygens, *de la Lum.* cap. 3. Euler, *Conj. Phys.* Phil. Transf. for 1800, p. 128. Young's Syllabus. Art. 382.)

Corollary 1. The same demonstrations prove the equality of the angles of reflection and incidence.

Corollary 2. It appears from experiments on the refraction of condensed air, that the ratio of the difference of the sines varies simply as the density. Hence it follows, by Schol. I. Prop. I. that the excess of the density of the ethereal medium is in the duplicate ratio of the density of the air; each particle co-operating with its neighbours in attracting a greater portion of it.

PROPOSITION VI.

When an Undulation falls on the Surface of a rarer Medium, so obliquely that it cannot be regularly refracted, it is totally reflected, at an Angle equal to that of its Incidence. (Phil. Transf. for 1800, p. 128.)

Prop. VI. Total reflection of undulation falling obliquely on the surface of a rarer medium.

Corollary. This phenomenon tends to prove the gradual increase and diminution of density at the surface terminating two mediums, as supposed in hypothesis IV; although Huygens has attempted to explain it somewhat differently.

PROPOSITION VII.

If equidistant Undulations be supposed to pass through a Medium, of which the Parts are susceptible of permanent Vibrations somewhat slower than the Undulations, their Velocity will be somewhat lessened by this vibratory Tendency; and, in the same Medium, the more, as the Undulations are more frequent.

Prop. VII. Undulations are retarded by passing through a medium vibrating with less frequency.

For, as often as the state of the undulation requires a change in the actual motion of the particle which transmits it, that change will be retarded by the propensity of the particle to continue its motion somewhat longer: and this retardation will be more frequent, and more considerable, as the difference

rence between the periods of the undulation and of the natural vibration is greater.

Doctrine of heat consisting in vibrations.

Corollary. It was long an established opinion, that heat consists in vibrations of the particles of bodies, and is capable of being transmitted by undulations through an apparent vacuum. (Newt. Opt. Qu. 18.) This opinion has been of late very much abandoned. Count Rumford, Professor Pictet, and Mr. Davy, are almost the only authors who have appeared to favour it; but it seems to have been rejected without any good grounds, and will probably very soon recover its popularity.

Suppose the parts of all bodies to vibrate;

Let us suppose that these vibrations are less frequent than those of light; all bodies therefore are liable to permanent vibrations slower than those of light; and indeed almost all are liable to luminous vibrations, either when in a state of ignition, or in the circumstances of solar phosphori; but much less easily, and in a much less degree, than to the vibrations of heat. It will follow from these suppositions, that the more frequent luminous undulations will be more retarded than the less frequent; and consequently, that blue light will be more refrangible than red, and radiant heat least of all; a consequence which coincides exactly with the highly interesting experiments of Dr. Herschel. (Phil. Trans. for 1800, p. 284.) It may also be easily conceived, that the actual existence of a state of slower vibration may tend still more to retard the more frequent undulations, and that the refractive power of solid bodies may be sensibly increased by an increase of temperature, as it actually appears to have been in Euler's experiments. (Acad. de Berlin. 1762. p. 328.)

then the more frequent luminous undulations will be most retarded. Blue light will be most refracted, and radiant heat least of all,

and refractive power will increase with the temperature.

Scholium. If, notwithstanding, this proposition should appear to be insufficiently demonstrated, it must be allowed to be at least equally explanatory of the phenomena with any thing that can be advanced on the other side, from the doctrine of projectiles; since a supposed accelerating force must act in some other proportion than that of the bulk of the particles; and, if we call this an elective attraction, it is only veiling under a chemical term, our incapacity of assigning a mechanical cause. Mr. Short, when he found by observation the equality of the velocity of light of all colours, felt the objection so forcibly, that he immediately drew an inference from it in favour of the undulatory system. It is assumed in the proposition,

This position explains the phenomenon at least as well as the projectile hypothesis.

proposition, that when light is dispersed by refraction, the corpuscles of the refracting substance are in a state of actual alternate motion, and contribute to its transimission; but it must be confessed, that we cannot at present form a very decided and accurate conception of the forces concerned in maintaining these corpuscular vibrations.

PROPOSITION VIII.

When two Undulations, from different Origins, coincide either perfectly or very nearly in Direction, their joint Effect is a Combination of the Motions belonging to each.

Prop. VIII.
Undulations that coincide in direction will combine.

Since every particle of the medium is affected by each undulation, wherever the directions coincide, the undulations can proceed no otherwise than by uniting their motions, so that the joint motion may be the sum or difference of the separate motions, accordingly as similar or dissimilar parts of the undulations are coincident.

I have, on a former occasion, insisted at large on the application of this principle to harmonics; (Phil. Trans. for 1800, p. 130.) and it will appear to be of still more extensive utility in explaining the phenomena of colours. The undulations which are now to be compared are those of equal frequency. When the two series coincide exactly in point of time, it is obvious that the united velocity of the particular motions must be greatest, and, in effect at least, double the separate velocities; and also, that it must be smallest, and if the undulations are of equal strength, totally destroyed, when the time of the greatest direct motion belonging to one undulation coincides with that of the greatest retrograde motion of the other. In intermediate states, the joint undulation will be of intermediate strength; but by what laws this intermediate strength must vary, cannot be determined without further data. It is well known that a similar cause produces in sound, that effect which is called a beat; two series of undulations of nearly equal magnitude co-operating and destroying each other alternately, as they coincide more or less perfectly in the times of performing their respective motions.

Effects when they are of equal frequency.

The beat in sound.

COROLLARY I. *Of the Colours of striated Surfaces.*

Boyle appears to have been the first that observed the colours of scratches on polished surfaces. Newton has not noticed Colours of striated surfaces explained,

ticed them. Mazeas and Mr. Brougham have made some experiments on the subject, yet without deriving any satisfactory conclusion. But all the varieties of these colours are very easily deduced from this proposition.

by the greater
length of ray
(or line of un-
dulation) from
the depressed
portion of sur-
face,

Let there be in a given plane two reflecting points very near each other, and let the plane be so situated that the reflected image of a luminous object seen in it may appear to coincide with the points; then it is obvious that the length of the incident and reflected ray, taken together, is equal with respect to both points, considering them as capable of reflecting in all directions. Let one of the points be now depressed below the given plane; then the whole path of the light reflected from it, will be lengthened by a line which is to the depression of the point as twice the cosine of incidence to the radius. Fig. 2, Plate VI.

which will occa-
sion alternations
of intensity and
remission in the
compound undu-
lation; accord-
ingly as the de-
pression causes
the undulations
of the succeed-
ing ray to coin-
cide with or op-
pose those of the
preceding ray.

If, therefore, equal undulations of given dimensions be reflected from two points, situated near enough to appear to the eye but as one, wherever this line is equal to half the breadth of a whole undulation, the reflection from the depressed point will so interfere with the reflection from the fixed point, that the progressive motion of the one will coincide with the retrograde motion of the other, and they will both be destroyed; but, when this line is equal to the whole breadth of an undulation, the effect will be doubled; and when to a breadth and a half, again destroyed; and thus for a considerable number of alternations; and, if the reflected undulations be of different kinds, they will be variously affected, according to their proportions to the various length of the line which is the difference between the lengths of their two paths, and which may be denominated the interval of retardation.

Experiment by
parallel strokes;

In order that the effect may be the more perceptible, a number of pairs of points must be united into two parallel lines; and, if several such pairs of lines be placed near each other, they will facilitate the observation. If one of the lines be made to revolve round the other as an axis, the depression below the given plane will be as the sine of the inclination; and, while the eye and luminous object remain fixed, the difference of the length of the paths will vary as this sine.

such as the fine
micrometers of
Coventry at the

The best subjects for the experiment are Mr. Coventry's exquisite micrometers; such of them as consist of parallel lines drawn

drawn on glass, at the distance of one five hundredth of an inch, are the most convenient. Each of these lines appears under a microscope to consist of two or more finer lines, exactly parallel, and at the distance of somewhat more than a twentieth of that of the adjacent lines. I placed one of these

distance of one five hundredth of an inch.

so as to reflect the sun's light at an angle of 45° , and fixed it in such a manner, that while it revolved round one of the lines as an axis, I could measure its angular motion; and I found, that the brightest red colour occurred at the inclinations $10\frac{1}{4}^\circ$, $20\frac{1}{4}^\circ$, 32° , and 45° ; of which the sines are as the numbers 1, 2, 3, and 4. At all other angles also, when the sun's light was reflected from the surface, the colour vanished with the inclination, and was equal at equal inclinations on either side.

When the angular position of these is varied they produce colours,

This experiment affords a very strong confirmation of the theory. It is impossible to deduce any explanation of it from any hypothesis hitherto advanced; and I believe it would be difficult to invent any other that would account for it. There is a striking analogy between this separation of colours, and the production of a musical note by successive echoes from equidistant iron palisades; which I have found to correspond pretty accurately with the known velocity of sound, and the distances of the surfaces.

which strongly and exclusively confirm the theory.

It is analogous to the musical echo from iron rails.

It is not improbable that the colours of the integuments of some insects, and of some other natural bodies, exhibiting in different lights the most beautiful versatility, may be found to be of this description, and not to be derived from thin plates. In some cases, a single scratch or furrow may produce similar effects, by the reflection of its opposite edges.

Versatile colours of some insects, &c. probably of this nature.

COROLLARY 2. *Of the Colours of thin Plates.*

When a beam of light falls on two parallel refracting surfaces, the partial reflections coincide perfectly in direction; and, in this case, the interval of retardation, taken between the surfaces, is to their distance as twice the cosine of the angle of refraction to the radius. For, in Fig. 3, Plate VI. drawing AB and CD perpendicular to the rays, the times of passing through BC and AD will be equal, and DE will be half the interval of retardation; but DE is to CE as the sine of DCE to the radius. Hence, that DE may be constant, or that the same colour may be reflected, the thickness CE must vary as the secant of the angle of refraction CED: which

Colours of thin plates explained

by the greater length of ray reflected from the posterior surface, which must produce an effect

agrees

similar to that of the striated surfaces; namely colours, agrees exactly with Newton's experiments; for the correction is perfectly inconsiderable.

which will be transmitted and reflected by turns if the thickness gradually vary, as in the Newtonian rings.

Breadth and duration of the colorific undulations determined,

and compared with those of sound.

Let the medium between the surfaces be rarer than the surrounding mediums; then the impulse reflected at the second surface, meeting a subsequent undulation at the first, will render the particles of the rarer medium capable of wholly stopping the motion of the denser, and destroying the reflection, (PROP. IV.) while they themselves will be more strongly propelled than if they had been at rest; and the transmitted light will be increased. So that the colours by reflection will be destroyed, and those by transmission rendered more vivid, when the double thicknesses, or intervals of retardation, are any multiples of the whole breadths of the undulations; and, at intermediate thicknesses the effects will be reversed; according to the Newtonian observations.

If the same proportions be found to hold good with respect to thin plates of a denser medium, which is indeed not improbable, it will be necessary to adopt the corrected demonstration of PROP. IV. but, at any rate, if a thin plate be interposed between a rarer and a denser medium, the colours by reflection and transmission may be expected to change places.

From Newton's measures of the thicknesses reflecting the different colours, the breadth and duration of their respective undulations may be very accurately determined; although it is not improbable, that when the glasses approach very near, the atmosphere of ether may produce some little irregularity. The whole visible spectrum appears to be comprised within the ratio of three to five, or a major sixth in music; and the undulations of red, yellow, and blue, to be related in magnitude as the numbers 8, 7, and 6; so that the interval from red to blue is a fourth. The absolute frequency expressed in numbers is too great to be distinctly conceived, but it may be better imagined by a comparison with sound. If a chord sounding the tenor \bar{c} , could be continually bisected 40 times, and should then vibrate, it would afford a yellow green light: this bring denoted by \bar{c}^{41} , the extreme red would be \bar{a}^{40} , and the blue \bar{d}^{42} . The absolute length and frequency of each vibration is expressed in the table; supposing light to travel in $8\frac{1}{2}$ minutes 500,000,000,000 feet.

Colours.

Colours.	Length of an Undulation in parts of an Inch, in Air.	Number of Undulations in an Inch.	Number of Undulations in a Second.
Extreme -	.0000266	37640	463 millions of millions.
Red - -	.0000256	39180	482
Intermediate	.0000246	40720	501
Orange - -	.0000240	41610	512
Intermediate	.0000235	42510	523
Yellow - -	.0000227	44000	542
Intermediate	.0000219	45600	561 ($= 2^{43}$ nearly)
Green - -	.0000211	47460	584
Intermediate	.0000203	49320	607
Blue - -	.0000196	51110	629
Intermediate	.0000189	52910	652
Indigo - -	.0000185	54070	665
Intermediate	.0000181	55240	680
Violet - -	.0000174	57490	707
Extreme -	.0000167	59750	735

Table of colorific undulations of the ether.

Scholium. It was not till I had satisfied myself respecting all these phenomena, that I found in Hooke's Micrographia, a passage which might have led me earlier to a similar conclusion. "It is most evident that the reflection from the under or further side of the body, is the principal cause of the production of these colours.—Let the ray fall obliquely on the thin plate, part therefore is reflected back by the first superficies,—part refracted to the second surface,—whence it is reflected and refracted again.—So that, after two refractions and one reflection, there is propagated a kind of fainter ray—," and, "by reason of the time spent in passing and repassing,—this fainter pulse comes behind the" former reflected "pulse; so that hereby, (the surfaces being so near together that the eye cannot discriminate them from one,) this confused or duplicated pulse, whose strongest part precedes, and whose weakest follows, does produce on the retina, the sensation of a yellow. If these surfaces are further removed asunder, the weaker pulse may become coincident with the" reflection of the second," or next following pulse, from the first surface, "and lag behind that also, and be coincident with the third, fourth, fifth, sixth, seventh, or eighth—; so that, if there be a thin transparent body, that from the greatest thinness require,

Quotation from Robert Hooke exhibiting a similar doctrine,

which was
printed seven
years before
Newton made
his experiments.

sitate to produce colours, does by degrees grow to the greatest thickness, the colours shall be so often repeated, as the weaker pulse does lose paces with its primary or first pulse, and is coincident with a subsequent "pulse. And this, as it is coincident, or follows from the first hypothesis I took of colours, so upon experiment have I found it in multitudes of instances that seem to prove it." (P. 65—67.) This was printed about seven years before any of Newton's experiments were made. We are informed by Newton, that Hooke was afterwards disposed to adopt his "suggestion" of the nature of colours; and yet it does not appear that Hooke ever applied that improvement to his explanation of these phenomena, or inquired into the necessary consequence of a change of obliquity, upon his original supposition, otherwise he could not but have discovered a striking coincidence with the measures laid down by Newton from experiment. All former attempts to explain the colours of thin plates, have either proceeded on suppositions which, like Newton's, would lead us to expect the greatest irregularities in the direction of the refracted rays; or, like Mr. Michell's, would require such effects from the change of the angle of incidence, as are contrary to the effects observed; or they are equally deficient with respect to both these circumstances, and are inconsistent with the most moderate attention to the principal phenomena.

COROLLARY 3. *Of the Colours of thick Plates.*

Colours of thick plates explained from the greater length of such rays of scattered light (compared with the principal ray) as pass through the first surface, and are reflected from the second with a different inclination.

When a beam of light passes through a refracting surface, especially if imperfectly polished, a portion of it is irregularly scattered, and makes the surface visible in all directions, but most conspicuously in directions not far distant from that of the light itself: and, if a reflecting surface be placed parallel to the refracting surface, this scattered light, as well as the principal beam, will be reflected, and there will also be a new dissipation of light, at the return of the beam through the refracting surface. These two portions of scattered light will coincide in direction; and, if the surfaces be of such a form as to collect the similar effects, will exhibit rings of colours. The interval of retardation is here, the difference between the paths of the principal beam and of the scattered light between the two surfaces; of course, wherever the inclination of the scattered light is equal to that of the beam, although in different

rent planes, the interval will vanish, and all the undulations will conspire. At other inclinations, the interval will be the difference of the secants from the secant of the inclination or angle of refraction of the principal beam. From these causes, all the colours of concave mirrors observed by Newton and others are necessary consequences; and it appears that their production, though somewhat similar, is by no means, as Newton imagined, identical with the production of those of thin plates.

COROLLARY 4. *Of Blackness.*

In the three preceding corollaries, we have considered the refracting and reflecting substances as limited by a mathematical surface; but this is perhaps never physically true. The ethereal atmospheres may extend on each side the surface as far as the breadth of one or more undulations; and, if they be supposed to vary equally in density at every part, the partial reflections from each of the infinite number of surfaces, where the density changes, will very much interfere with each other, and destroy a considerable portion of the reflected light, so that the substance may become positively black; and this effect may take place in a greater or less degree, as the density of the ethereal atmosphere varies more or less equably; and, in some cases, particular undulations being more affected than others, a tinge of colour may be produced. Accordingly, M. Bouguer has observed a considerable loss of light, and in some instances a tinge of colour, in total reflections at the surface of a rarer medium.

COROLLARY 5. *Of Colours by Inflection.*

Whatever may be the cause of the inflection of light passing through a small aperture, the light nearest its centre must be the least diverted, and the nearest to its sides the most; another portion of light falling very obliquely on the margin of the aperture, will be copiously reflected in various directions; some of which will either perfectly or very nearly coincide in direction with the unreflected light, and, having taken a circuitous route, will so interfere with it, as to cause an appearance of colours. The length of the two tracks will differ the less, as the direction of the reflected light has been less changed by its reflection, that is, in the light passing nearest

to

to the margin; so that the blues will appear in the light nearest the shadow. The effect will be increased and modified, when the reflected light falls within the influence of the opposite edge, so as to interfere with the light simply inflected by that also.

More particular examination of the consequences.

But, in order to examine the consequences more minutely, it will be convenient to suppose the inflection caused by an ethereal atmosphere, of a density varying as a given power of the distance from a centre, as in the eighth proposition of the last Bakerian Lecture. (Phil. Trans. for 1801, p. 83.) Putting $r = 3$, and $x = \frac{1}{2}$, I have constructed a diagram, (Fig. 4,) which shows, by the two pair of curves, the relative position of the reflected and unreflected portions of any one undulation at two successive times, and also, by shaded lines drawn across, the parts where the intervals of retardation are in arithmetical progression, and where similar colours will be exhibited at different distances from the inflecting substance. The result fully agrees with the observations of Newton's third book, and with those of later writers. But I do not consider it as quite certain, until further experiments have been made on the inflecting power of different substances, that Dr. Hooke's explanation of inflection, by the tendency of light to diverge, may not have some pretensions to truth. I am sorry to be obliged to recall here the assent which, at first sight, I was induced to give to a supposed improvement of a late author. (Phil. Trans. for 1800, p. 128.)

Scholium. In the construction of the diagram, it becomes necessary to find the time spent by each ray in its passage.

Since the velocity was denoted by $x \frac{1}{r}$, on the supposition

of a projectile, it will be as $x \frac{1}{r}$ on the contrary supposition, (Phil. Trans. for 1801, p. 27. *Schol.* 2. Prop. I.) and the

fluxion of the distance described being $\frac{\dot{x}}{\sqrt{1-yy}}$, that of the

time will be $\frac{\frac{x}{r} \frac{1}{x}}{\sqrt{1-yy}}$ or $\frac{rs}{1-r} \cdot \frac{\dot{y}}{yy \cdot \sqrt{1-yy}}$, of which the flu-

ent is $\frac{x}{1-r} \cdot \frac{s}{y} \cdot \sqrt{1-yy}$. Therefore, with the radius $x \frac{1}{r}$,

describe

describe a circle concentric with the surfaces of the inflecting atmosphere, then the angle described by the ray during its passage through the atmosphere, will always be to the angle subtended by the line cut off by this circle from the incident ray produced, in the ratio of r to $r-i$; and the time spent in this passage, will be in the same ratio to the time that would have been spent in describing this intercepted portion with the

initial velocity. For y , being equal to $sx \frac{1}{r-i}$, is the sine of the inclination of the incident ray to the radius, where it meets this circle; therefore by the proposition quoted, the angle described is in a given ratio to the angle at the centre,

which is the difference of the inclinations. Making $x^2 = \frac{1}{r}$

or $\frac{s}{y}$ radius, the sine, instead of y , becomes s , and the cosine

$$\sqrt{\frac{ss}{yy} - ss}, \text{ or } \frac{s}{y} \sqrt{1 - yy}, \text{ and, when } y = ss, \sqrt{1 - ss};$$

therefore the line intercepted is to the difference of the fluents as r to $r-1$. (See also Young's Syllabus, Art. 372.)

PROPOSITION IX.

Radiant Light consists in Undulations of the luminiferous Ether.

Conclusion. Radiant light consists in undulations of the ether.

This proposition is the general conclusion from all the preceding; and it is conceived that they conspire to prove it in as satisfactory a manner as can possibly be expected from the nature of the subject. It is clearly granted by Newton, that there are undulations, yet he denies that they constitute light; but it is shown in the three first Corollaries of the last Proposition, that all cases of the increase or diminution of light are referable to an increase or diminution of such undulations, and that all the affections to which the undulations would be liable, are distinctly visible in the phenomena of light; it may therefore be very logically inferred, that the undulations are light.

A few detached remarks will serve to obviate some objections which may be raised against this theory.

1. Newton has advanced the singular refraction of the Iceland crystal, as an argument that the particles of light must be projected corpuscles; since he thinks it probable that the different sides of these particles must be differently attracted by the crystal, and since Huygens has confessed his inability to

Remarks. Newton without giving a reason rejected the law of Huygens in explaining the phenomena of Iceland crystal.

account

account in a satisfactory manner for all the phenomena. But, contrarily to what might have been expected from Newton's usual accuracy and candour, he has laid down a new law for the refraction, without giving a reason for rejecting that of Huygens, which Mr. Haüy has found to be more accurate than Newton's; and, without attempting to deduce from his own system any explanation of the more universal and striking effects of doubling spars, he has omitted to observe that Huygens's most elegant and ingenious theory perfectly accords with these general effects, in all particulars, and of course derives from them additional pretensions to truth: this he omits, in order to point out a difficulty, for which only a verbal solution can be found in his own theory, and which will probably long remain unexplained by any other.

Michell's experiments on the momentum of light explained, without supposing any projection.

They did not succeed with Bennet.

Latent light and heat not inconsistent with the doctrine of vibrations.

2. Mr. Michell has made some experiments, which appear to show that the rays of light have an actual momentum, by means of which a motion is produced when they fall on a thin plate of copper delicately suspended. (Priestley's Optics.) But, taking for granted the exact perpendicularity of the plate, and the absence of any ascending current of air, yet since, in every such experiment, a greater quantity of heat must be communicated to the air at the surface on which the light falls than at the opposite surface, the excess of expansion must necessarily produce an excess of pressure on the first surface, and a very perceptible recession of the plate in the direction of the light. Mr. Bennet has repeated the experiment, with a much more sensible apparatus, and also in the absence of air; and very justly infers from its total failure, an argument in favour of the undulatory system of light. (Phil. Transf. for 1792, p. 87.) For, granting the utmost imaginable subtilty of the corpuscles of light, their effects might naturally be expected to bear some proportion to the effects of the much less rapid motions of the electrical fluid, which are so very easily perceptible, even in their weakest states.

3. There are some phenomena of the light of solar phosphori, which at first sight might seem to favour the corpuscular system; for instance, its remaining many months as if in a latent state, and its subsequent re-emission by the action of heat. But, on further consideration, there is no difficulty in supposing the particles of the phosphori which have been made to vibrate by the action of light, to have this action abruptly suspended

suspended by the intervention of cold, whether as contracting the bulk of the substance or otherwise; and again, after the restraint is removed, to proceed in their motion, as a spring would do which had been held fast for a time in an intermediate stage of its vibration; nor is it impossible that heat itself may, in some circumstances, become in a similar manner latent. (Nicholson's Journal, Vol. II. p. 399.) But the affections of heat may perhaps hereafter be rendered more intelligible to us; at present, it seems highly probable that light differs from heat only in the frequency of its undulations or vibrations; those undulations which are within certain limits, with respect to frequency, being capable of affecting the optic nerve, and constituting light; and those which are slower, and probably stronger, constituting heat only; that light and heat occur to us, each in two predicaments, the vibratory or permanent, and the undulatory or transient state; vibratory light being the minute motion of ignited bodies, or of solar phosphori, and undulatory or radiant light the motion of the ethereal medium excited by these vibrations; vibratory heat being a motion to which all material substances are liable, and which is more or less permanent; and undulatory heat that motion of the same ethereal medium, which has been shown by Mr. King, (*Morsels of Criticism*, 1786, p. 99,) and M. Pictet, (*Essai de Physique*, 1790, also in Saussure's *Voyage dans les Alpes*, 1786,) to be as capable of reflection as light, and by Dr. Herschel to be capable of separate refraction. (*Phil. Transf.* for 1800, p. 284.) How much more readily heat is communicated by the free access of colder substances, than either by radiation or by transmission through a quiescent medium, has been shown by the valuable experiments of Count Rumford. It is easy to conceive that some substances, permeable to light, may be unfit for the transmission of heat, in the same manner as particular substances may transmit some kinds of light, while they are opaque with respect to others.

On the whole it appears, that the few optical phenomena which admit of explanation by the corpuscular system, are equally consistent with this theory; that many others, which have long been known, but never understood, become by these means perfectly intelligible; and that several new facts are found to be thus only reducible to a perfect analogy with other facts, and to the simple principles of the undulatory system.

Light and heat differ only in the frequency of undulation or vibration,

capable in both cases of the same modifications.

This theory explains all that is solved by the corpuscular system, and much more.

system. It is presumed, that henceforth the second and third books of Newton's Optics will be considered as more fully understood than the first has hitherto been; but, if it should appear to impartial judges, that additional evidence is wanting for the establishment of the theory, it will be easy to enter more minutely into the details of various experiments, and to show the insuperable difficulties attending the Newtonian doctrines, which, without necessity, it would be tedious and invidious to enumerate. The merits of their author in natural philosophy, are great beyond all contest or comparison; his optical discovery of the composition of white light, would alone have immortalised his name; and the very arguments which tend to overthrow his system, give the strongest proofs of the admirable accuracy of his experiments.

Experiment in which the corpuscular theory ought to exhibit distortion.

Sufficient and decisive as these arguments appear, it cannot be superfluous to seek for further confirmation; which may with considerable confidence be expected, from an experiment very ingeniously suggested by Professor Robison, on the refraction of the light returning to us from the opposite margins of Saturn's ring; for, on the corpuscular theory, the ring must be considerably distorted when viewed through an achromatic prism: a similar distortion ought also to be observed in the disc of Jupiter; but, if it be found that an equal deviation is produced in the whole light reflected from these planets, there can scarcely be any remaining hope to explain the affections of light, by a comparison with the motions of projectiles.

IV.

An Analysis of a Mineral Substance from North America, containing a Metal hitherto unknown. By CHARLES HATCHETT, Esq.

(Concluded from Page 138.)

H.

The white precipitate was little affected by distillation of the sulphuric and nitric acids.

THE white precipitate, when distilled with four parts of sulphur, remained pulverulent, and, from white, was only changed to a pale ash colour.

Nitric acid was digested on the powder, and, being heated, afforded some nitrous gas; after this, the powder became white, and in every respect recovered its original properties.

Before

I.

Before I conclude this section, I must observe, that when the olive-green precipitates, obtained by prussiate of potash, were digested in an alkaline lixivium, they were decomposed; for the alkali combined with the prussic acid, and with a small part of the white matter; but the greater part of the latter remained undissolved, in the same white flocculent state which was noticed when the alkaline combinations were mentioned.

The orange-coloured precipitates, formed by tincture of galls, were also decomposed when digested in boiling nitric acid; and the white matter was recovered in its original state.

§ III. REMARKS.

The preceding experiments shew, that the ore which has been analysed, consists of iron combined with an unknown substance, and that the latter constitutes more than three-fourths of the whole. This substance is proved to be of a metallic nature, by the coloured precipitates which it forms with prussiate of potash, and with tincture of galls; by the effects which zinc produces, when immersed in the acid solutions; and by the colour which it communicates to phosphate of ammonia, or rather to concrete phosphoric acid, when melted with it.

Moreover, from the experiments made with the blow-pipe, it seems to be one of those metallic substances which retain oxygen with great obstinacy, and are therefore of difficult reduction.

It is an acidifiable metal; for the oxide reddens litmus paper, expels carbonic acid, and forms combinations with the fixed alkalis. But it is very different from the acidifiable metals which have of late been discovered; for,

1. It remains white when digested with nitric acid.
2. It is soluble in the sulphuric and muriatic acids; and forms colourless solutions, from which it may be precipitated, in the state of a white flocculent oxide, by zinc, by the fixed alkalis, and by ammonia. Water also precipitates it from the sulphuric solution, in the state of a sulphate.
3. Prussiate of potash produces a copious and beautiful olive-green precipitate.

Obf. The ore is iron, with three parts of the white matter. The latter is metallic; for it is precip. by prussiate, and by galls, and by zinc, and it colours phosf. acid by fusion.

It is of difficult reduction,

Differs from all other the acidifiable metals, in the properties here enumerated.

other substances, which are now considered as simple, primitive, and distinct bodies, will be found to be compounds. Yet I only entertain and state this opinion as a probability; for, until an advanced state of chemical knowledge shall enable us to compose, or at least to decompose, these bodies, each must be classed and denominated as a substance *ſui generis*. Considering, therefore, that the metal which has been examined is so very different from those hitherto discovered, it appeared proper that it should be distinguished by a peculiar name; and, having consulted with several of the eminent and ingenious chemists of this country, I have been induced to give it the name of Columbium.

POSTSCRIPT.

It appears proper to mention some unsuccessful attempts which I have lately made to reduce the white oxide.

Reduction. The white powder was only blackened by the strong heat with charcoal. Fifty grains were put into a crucible coated with charcoal; and being covered with the same, the crucible was closely luted, and was exposed to a strong heat, in a small wind furnace, during about one hour and an half. When the crucible was broken, the oxide was found in a pulverulent state; and, from white, was become perfectly black.

The phosphuret. In order to form a phosphuret, some phosphoric acid was poured upon a portion of the white oxide; and, being evaporated to dryness, the whole was put into a crucible coated with charcoal, as above described. The crucible was then placed in a forge belonging to Mr. Chenevix, and a strong heat was kept up for half an hour.

The inclosed matter was spongy, and of a dark brown; it in some measure resembled phosphuret of titanium.

After this we wished to try the effect of a still greater heat; but in this experiment the crucible was melted.

The above experiments shew, that the white oxide, like several other metallic substances, may be deoxidated to a certain degree, without much difficulty, but that the complete reduction of it is still far from being easily effected.

V.

On the Effect of Sound upon the Barometer. By Sir HENRY C. ENGLEFIELD, Bart. F. R. S. (From the Journals of the Royal Institution, No. 9.)

DURING the time I spent at Brussels in the year 1773 and 1774, it occurred to me, that the effect of sound on the barometer had not, to my knowledge, been attended to; and that it was by no means certain, whether that instrument was capable of being sensibly affected by those elastic vibrations caused in the atmosphere, by the percussion of a sonorous body. I thought the idea worthy of being pursued, and the means of making satisfactory experiments were most opportunely in my power.

Whether the barometer be affected by sonorous undulation.

The sound of a very large bell appeared to me the most powerful, and at the same time to be approached with the greatest security and ease to the observer. The explosion of artillery, besides the very disagreeable smoke and danger of the recoil, might be objected to, on account of the sudden production of elastic and heated vapour, which might, independent of the sound, instantaneously alter the state of the atmosphere, and thereby lead the observer into very great and unavoidable errors.

The bell preferred for experiments.

Every one who has been in the Low Countries must know, that very large bells, and immense numbers of them, are the pride of their churches; and that they are rung quite out, not tolled, on every great festival. The great bell of the collegiate church of St. Gudula, at Brussels, weighs, as I was told, sixteen thousand pounds, and on this I determined to found my experiment.

Large bells in the Netherlands.

Two objections only could be made to the result of this experiment, the one, that the motion of the bell might cause a vibration in the walls of the building, which would hinder the placing the barometer in a state of repose; the other, that the swinging so large a mass with a considerable degree of velocity, might of itself agitate the air so as to cause vibrations in the mercury totally independent of sound.

Whether these would produce irregularity by agitating the building or the air.

The strength of the walls of the steeple, and the manner of hanging the bell, which was contained in a frame of timber, founded

Observations.

founded on a strong vault, and totally independent of the wall of the steeple, might alone have answered the first of these objections; but happily a most complete and satisfactory answer to both of them, was furnished by the manner in which the bell was rung.

Preparation for experiment.

As the bell was to ring out full in an instant, at a signal given from below, it is necessary to have it in motion some time beforehand; and during that time, the clapper is fixed to one side by a strong stick crossing the mouth of the bell, which, at the signal, is pulled out by the hand of a person placed for that purpose. If then, our barometer shewed no variation during all this time, we were absolutely certain that whatever motion was perceived afterwards, was wholly owing to the sound.

Mr. Pigott, who was then at Brussels, was kind enough to lend me one of his barometers, made by Ramsden, and his son made the following observations jointly with myself:

Narrative.

At two o'clock in the afternoon of the 1st of November 1773, we went into the northwest tower of St. Gudula's church, and having fixed the barometer firmly in the opening of a window, not above seven feet from the bottom of the bell, we waited quietly for its ringing.

The height of the mercury before the bell began to swing, as observed by Mr. Pigott, was 29.478 inches. The bell being in full swing, no alteration whatever was perceptible.

The mercury was elevated by the sound of a large bell.

The instant that the clapper was loosed the mercury leaped up, and continued that sort of springing motion, at every stroke of the clapper, during the whole time of the ringing of the bell. These were our observations:

During the ringing of the bell, Mr. P.					-	29.469
During the ringing, by myself.						
Highest	-	-	-	-	-	29.480
Lowest	-	-	-	-	-	29.474
Highest	-	-	-	-	-	29.482
Lowest	-	-	-	-	-	29.472

These observations were made with the greatest attention; and, considering their delicacy and the difficulty of observing, agree very nearly. They appear to give from 6 to 10 thousandths of an inch for the effect of this sound on the barometer. It is to be observed, that Mr. Pigott, in general, estimated the

the height of the mercury about 5 thousandths lower than myself, which brings our observations to a very near agreement. The following observations prove this:

On the top of the tower, Mr. P.	-	-	29.424
Ditto, by me	-	-	29.430
At the foot of the tower, Mr. P.	-	-	29.639
Ditto, by me	-	-	29.642
In the court of the English Nuns, by Mr. P.	-	-	29.676
Ditto, by me	-	-	29.682

And I should think that the difference of eyes may frequently cause such a variation among different observers; at least in delicate observations, it will be always prudent to make the experiment.

NOTE BY DR. YOUNG.

THESE observations appear to agree too well with each other, to allow us to doubt of their accuracy. It therefore becomes necessary to inquire into the causes of the different heights of the barometer. It is indeed barely possible, that a sudden stroke of the clapper on the bell might produce a greater agitation of the building than the preceding alternate motion of the bell itself: but this explanation cannot be called satisfactory. It is certain, that there was neither more nor less air in the tower while the bell was sounding, than while it was silent; the mean density of the air could therefore not have been changed; and if the alternate motions of the particles of air which constitute sound, had taken place by equal degrees, and with equal velocities, in each opposite direction, there is no reason to suppose that the increase of pressure on the surface of the mercury, at one instant, could have tended to raise it, more than the decrease of pressure, in the opposite state of the undulation, would have depressed it. But the same consequence does not follow, if we conceive the motion of the air, in advancing, to be more rapid, but of shorter continuance, than its retrograde motion. For if the wind blew for one hour with a velocity of 4, and the same air returned, in the course of two hours, with a velocity of 2, an obstacle upon which it had acted in both directions would not be found in its original place; for the action of the wind upon an obstacle is as the square

Observations by
Dr. Young.

The mean density of the air was not changed.

If the motion of advance were more rapid than of return in the undulations, it would have the effect of a pressure.

square of the velocity, and the time would not compensate for the difference of force. It is therefore easy to suppose, that the law of the bell's vibration was in this experiment such, that the air advanced towards the barometer with a greater velocity than it receded, although for a shorter time; and that hence the whole effect was the same as if the mean pressure of the air

This law might easily result from subordinate vibrations, combining with the principal.

Slight repulsions from this cause in elastic mediums. Whether gravity be so caused.

had been increased. Such a law might easily result from a combination of a more regular principal vibration with one or more subordinate ones, in different relations; and similar cases may sometimes be observed in the vibrations of chords. Here we find a slight degree of repulsion, in consequence of the undulations of an elastic medium. Dr. Hooke attempted to explain the phenomena of attraction by means of similar undulations of an ether, which he supposed to be the medium serving for the communication of heat; but it must be confessed, that the conjecture has little appearance of probability.

VI.

On the Expansion of Carbonated Hydrogen by Electricity. From a Correspondent.

To Mr. NICHOLSON.

S I R,

Expansion of carb. hydrogen by electricity not yet explained.

I VENTURE to trouble you with a few remarks on a phenomenon in chemistry, the expansion of carbonated hydrogen gas on the electric spark being passed through it. None of the explanations of this circumstance which I have met with, appear to me to be satisfactory; and I have stated a few objections which occurred to me on considering them. If you think them of sufficient weight to fill a corner of your ably-conducted Journal, some correspondent of greater abilities may perhaps give a new explanation of the fact. But if you think them beneath your notice, and unworthy of a place among the excellent communications with which your numbers are filled, I have only to beg that you will pardon the presumption of a very young chemist, who, by this first attempt, has perhaps only exposed his own ignorance.

I am, Sir,

Your's with much respect,

Edin. June 8, 1802.

G. H.

Gn

On the Expansion of carbonated Hydrogen Gas.

This fact was first observed by Dr. Austin. On passing the electric spark through a quantity of carbonated hydrogen gas, he found that the gas was permanently dilated to more than twice its original bulk. He concluded that this remarkable expansion could only be owing to the evolution of hydrogen gas. Upon burning the air thus expanded in oxygen gas, he found that it required more oxygen for its combustion than an equal quantity of carbonated hydrogen gas, which had not been expanded by the electric spark. *An addition therefore had been made to the combustible matter*; for the quantity of oxygen necessary to complete the combustion of any body, is always proportional to the quantity of that body. He concluded from these experiments, that he had decomposed the carbon which had been dissolved in the hydrogen, and that carbon was composed of hydrogen and azote, some of which was always found in the vessel after the combustion.

Dr. Austin first observed that carbon, hydrogen is expanded to double by electricity.

His theory that carbon is hydrogen and azote.

If Dr. Austin had more attentively considered the circumstances of these experiments, he would probably have been prevented from drawing this conclusion. *The quantity of combustible matter had been increased.* Now, if the expansion of the carbonated hydrogen gas was owing merely to the decomposition of carbon, no such increase ought to have taken place, but rather the contrary; for the carbon, which was itself a combustible substance, was resolved into two ingredients, hydrogen and azote, only the first of which burnt on the addition of oxygen, and the application of heat. And besides, if the carbon had been resolved into hydrogen and azote, the product of the combustion could only have been a greater quantity of water, with a residuum of azote; for the hydrogen evolved by the dilatation of the carbonated hydrogen gas, combining with the additional oxygen, must have formed an additional quantity of water. But it is a fact, which the Doctor does not seem to have attended to, that besides water, *carbonic acid gas* is produced from the combustion of the expanded carbonated hydrogen. Whence, then, comes the carbonic acid gas?

Objections. Hydrogen and azote are not more combustible than carbon: and the product is not mere water and azote, but also carbonic acid gas.

Mr. Henry*, who repeated Dr. Austin's experiments with great accuracy, found that he was correct with regard to the

Mr. Henry found that the expansion is

* Philof. Transact. 1797.

produced by hydrogen from decomposed water.

increase of combustible matter. He also found, that the expansion could not be carried beyond a certain point, about twice the original bulk of the gas. Upon burning separately by means of oxygen, two equal portions of carbonated hydrogen gas, one of which had been expanded by electricity to twice its original bulk, the other not, he found that each of them produced precisely the same quantity of carbonic acid gas; a proof, that the carbon in both remained the same, and that the hydrogen could not have been produced from it. He concluded therefore, that the evolution of the hydrogen is produced from the decomposition of the water with which hydrogen is always more or less impregnated; to prove which, he took hydrogen gas, from which he expelled as much water as possible, and found that the electric spark produced a very small dilatation; but on admitting a drop or two of water, the expansion went on as usual. It is easily seen how this decomposition is effected. Carbon at a high temperature has a greater affinity for oxygen than hydrogen gas; when the temperature is raised, therefore, by the electric spark, the carbon unites with the oxygen of the water, forming carbonic acid gas, and the hydrogen is evolved. As to the azote, it must have been produced from the admission of atmospheric air into the process.

But this discovery does not account for the greater quantity of oxygen required to burn the expanded gas.

Although this theory of Mr. Henry seems to prove the production of hydrogen from the decomposition of the water contained in the gas; still, however, it is no more able to account for the increase of combustible matter than that of Dr. AUSTIN. When the gas is dilated, hydrogen and oxygen are evolved from the water. The hydrogen goes to increase the bulk of the gas, and the oxygen unites with the carbon. Expanded carbonated hydrogen gas, then, contains hydrogen, carbon, and carbonic acid gas. In combustion, the evolved hydrogen, in order to form water, requires a quantity of additional oxygen, precisely the same as that with which it was combined before decomposition of the water: and on the other hand, the carbon requires precisely the same quantity less, as part of it is already rendered incombustible, by being combined with the oxygen of the water. Therefore, according to Mr. Henry's theory, a quantity of expanded carbonated hydrogen gas requires the same quantity of oxygen for its combustion, as an equal quantity that has not been expanded: and

and consequently they both contain the same quantity of combustible matter; for in proportion as the combustible matter is increased by the formation of hydrogen, in the same proportion it is diminished by the formation of carbonic acid gas. But this is observed to be contrary to fact; for carbonated hydrogen gas when expanded does actually contain more combustible matter than before its expansion. It would appear then, that a satisfactory explanation of this increase of combustible matter has not yet been given.

G. H.

VII.

A new Process for claying Sugars, proposed by CIT. HAPÉL-LACHENAIE, Chief Apothecary of the Military Hospitals of Guadaloupe, to the Agents of the Consuls of the French Republic in the Windward Islands.*

BEING called upon by the scientific mission confided to me Introduction. by the Government, to employ myself in every kind of research which may prove interesting to the cultivation and productions of this colony, I have thought it my duty to endeavour to discover a simple, easy, and cheap method of dispensing with the pottery which is used in the fabrication of clayed sugars.

There was even an urgent necessity that I should direct my Want of Pans for claying sugars in St. Domingo, attention to these objects, because most of the proprietors of these colonies, being at this moment in want of the forms and pots hitherto considered as indispensibly necessary for the claying sugars, are obliged to wait till the small number of pots they possess shall be cleared of the sugar they contain before they can fabricate more.

The delays occasioned by this inconvenience are the more obviously productive of great loss. prejudicial to the inhabitants in general, because they are obliged, on this account, to defer cutting their canes when at the most advantageous time of their growth; and as they cannot perform this at the most favourable season, their losses are incalculable by suffering the canes to grow old, and finishing

* From the *Annales de Chimie*, XL. 73.

their

their gathering in the season when they afford very little sugar, in comparison with what they would have yielded if they had been cut in time.

While this retardation diminishes the immediate product, it has an influence on the vigour of the succeeding shoots, as I have observed, and renders their advancement less speedy. And by this means it exposes the farmer of national plantations to fail in payment of the terms of their location.

Experiments for a new process, I shall not dwell upon the various experiments I made before my success was complete, but shall confine myself to those means which have best succeeded with me, and to which I have confined myself in the subsequent practice in my sugar-work.

for disposing the sugar to be clayed in large vessels. This process consists in disposing the sugar in receptacles of 12,937 cubic feet, each containing 26 ordinary forms, and to clay it in these same receptacles, of which I shall proceed to give a description :

First used by Citizens Boucherie at Paris, The priority of the method which I propose belongs to Cits. Boucherie, who first adopted it in their refinery of Bercy near Paris. They were, as far as I know, the first who constructed vessels to clay the raw sugars they received from our colonies previous to their refining them. But though this invention is undoubtedly theirs, I also have a claim as to the degree of improvement which their vessel did not possess, but which I have given them by rendering them more convenient, advantageous, and economical *.

Description of the apparatus of Citizens Boucherie. The vessels of Citizens Boucherie were, as nearly as I can recollect, about 15 or 18 inches deep, and five feet wide. They were square, and the bottom of each was perforated with a great number of small holes for the discharge of the syrup, which fell into a second vessel less deep, but of the same dimensions as the others as to their width. This second vessel was lined with thin metallic plates, of an alloy invented by Mr. Hilkerdeau, a Spanish chemist. The upper vessels

* I attended their operations in the year 1784, before I came to this colony. I had also the advantage at that time of giving several lectures in their establishment to some cultivators of St. Domingo and other sugar islands, to facilitate their knowledge of the doctrine and processes of that interesting manufacture which they frequently visited.

were

were supported on their edges by a level and firm framing, the inferior vessels stood on the floor.

The upper vessels were filled with raw sugar intended to be clayed, which was first well divided or crumbled in order to its equal distribution. This was levelled and compressed as equally as possible, to form what we commonly call, the bottom (*les fonds*). Upon this mass the diluted and prepared earth, properly adapted to the operation, was very carefully poured.

Charged with raw sugar.

Though this process may appear very simple at first sight, it nevertheless presents difficulties which could scarcely be overcome in that manufactory, and which would be almost impossible to surmount in ours, where the men employed in this business have neither the understanding nor the skill of those of Citizens Boucherie; and it was not till they had practised for some time that they became able to perform it without difficulty.

This simple process is liable to objections.

The first operation in which a failure may be made for want of skill, is that of levelling with a trowel an elastic surface of 25 square feet. The second is to compress equally that surface in order to give solidity to the bottom, that the diluted earth may extend to the same thickness on all sides, and find that surface so close as to admit the penetration of the water only.

It is difficult to level and press the sugar in the large vessels.

All those who are engaged in the manufacture of sugar must be aware, that if any inequality exists in the levelling of the bottom, the water which gradually leaves the earth naturally flows to the lowest place, where, if the compression be not every where the same, the water insinuates itself into the most porous part; and in either of these cases, this fluid being conducted with the earth towards a single point, is collected in sufficient abundance to dissolve the sugar at that place, and form what is called a fox (*un renard*.) When this happens the operation of claying fails, for it is known that the portion of earth retained at the surface acts little upon the rest of the mass of sugar. This inconvenience, which occasions a great loss in the product, is also found to change its quality, which is worse on this account.

Mischievous consequences of bad levelling or pressing.

The water flows to the lowest place and dissolves the sugar; while the rest is not full purified.

In the construction of the cases or vessels of Citizens Boucherie, which cannot be removed, other inconveniences are found which do not exist in mine. One of these is the difficulty

The sugar is not easily taken out.

culty of taking out the sugar after its drainage. A second is, that the holes in their bottom are too small, which renders them liable to be obstructed, and the process to be greatly retarded.

(To be concluded in our next.)

VIII.

Description of the Crystalline Forms of the Anhydrous Sulphate of Lime, with some Observations on this Substance. By M. Le COMTE DE BOURNON, Member of the Royal and Linnæan Societies of London. Translated from the Original, communicated by the Author.*

Sulphate of Lime containing no water is a new object in mineralogy.

GYPSUM, or the sulphate of lime deprived of water, is a new object in mineralogy: scarcely has the science as yet cast a glance upon this substance, and none of its crystalline forms have hitherto been determined. Having had opportunities of examining some of these forms, and at the same time of comparing a number of specimens of this substance, procured from different countries, I felt myself induced to lay before the Public my observations upon this subject, thinking they might contribute to throw some light upon the nature of several stones, with which we are not yet perfectly acquainted.

Its primitive form is a right prism with rectangular bases.

The primitive form of this substance, which has been designated by the name of Anhydrous Sulphate of Lime by the Abbé Haüy, is a rectangular paralelopiped, as had been presumed by this able mineralogist; but it does not appear that this paralelopiped can be the perfect cube, as is indicated by the habitual form of the crystals, which when most simple, is always a right prism with rectangular bases, having two opposite sides broader than the two others (Fig. 1, Plate XI.), and seems equally to be indicated by the circumstances which accompany this form. The mechanical division is very easy upon all the faces of this crystal; but the longitudinal straight faces present, in this respect, a little more resistance than the others.

Easily divided.

And afterwards revised by him.

The

The broad faces, as well as those which terminate this rectangular prism, have generally a very brilliant lustre; the longitudinal narrow ones are duller, and very frequently striated in the direction of their length. The broad sides are besides characterised by a pearly reflection, similar to that which is peculiar to the zeolite stilbite, and this brilliancy remains even after those faces have been divided: they also habitually exhibit the intersection (*entrecroissement*) at right angles of the joints belonging to the other faces.

This crystal presents, along its longitudinal edges, a decrease which replaces each of these sides by a plane unequally inclined upon the adjacent faces: it forms with the broad sides of the prism an angle of 130° , and of 140° with the narrow sides, (Fig. 2.) The prism is therefore octahedral, and has four edges of 130° , and four others of 140° . Secondary forms.
Octahedral prism.

These new planes frequently join each other, upon the narrow sides of the prism, and they then convert the octahedral into a hexahedral form, having four edges of 130° , and the two others of 100° (Fig. 3.) The pearly sides frequently then continue to be the broadest; in other instances, the six sides of the prism are equal, or nearly equal with each other. I have not yet seen any secondary faces placed upon the terminal edges. Hexahedral prism.

The crystals upon which this description is founded are of considerable magnitude: some are more than an inch in length. They are of a fine flesh colour, and formed part of one of the specimens brought from the Tyrol, in which the anhydrous sulphate of lime is mixed with muriate of soda or common salt; but as this salt is intirely foreign to it, and only interposed between its parts, frequently even in a manner that is very perceptible to the eye, the form of these crystals ought to be considered as being really that of the pure anhydrous sulphate of lime. Besides, I have seen in Mr. Greville's cabinet at London, a specimen in which this substance presents exactly the same aspect, and the same colour, and which at the same time is totally destitute of salt. In this, the anhydrous sulphate of lime is confusedly intermingled with ætine of a pale green colour, with some parts of cupreous pyrites, and of the black and very magnetic oxide of iron. The locality of this interesting specimen has not been preserved; but there is every reason to presume that it came from Sweden. Foreign admixture of muriate of soda.

In

Groups of crystals;

In the Tyrolese specimens, similar to that which afforded the crystals which I have just described, these are for the most part strongly engaged and applied against each other, crossing each other in different directions; some of them, however, are insulated. But as many of them are united by their breadth or pearly faces, they have no very strong adhesion, and it is always sufficiently easy to separate them with the edge of a knife. These specimens frequently exhibit in their mass more or less considerable portions of pure common salt; several of their crystals are considerably transparent.

Peculiarity.
The presence of sulphur of antimony and quartz.

A piece of this substance which I procured for Mr. Chenevix, for the purpose of analysing it, exhibited a peculiarity which deserves to be remarked. On breaking some of its crystals, small needles of sulphuret of antimony were perceived within it, adhering for the most part to small groups of crystals of quartz. Not a trace of either of these two substances was found in the other parts that were subjected to analysis: the same was the case with the carbonate of lime, which Mr. Klaproth has indicated at $\frac{1}{16}$ of the analysis which he made of it, and which, undoubtedly, was likewise only an extraneous or interposed substance.

Quartz in another specimen.

In like manner, in the stone of Vulpino, observed and described very accurately and carefully by Messrs. Fleurieu and Bellevue, in the 2d Vol. of the Journal de Physique for 1798, the anhydrous sulphate of lime is mixed with interposed particles of quartz, which, according to the analysis made of it by Mr. Vauquelin, are in the proportion of $\frac{1}{16}$ to its whole mass. I am indebted to the friendship of Mr. Fleurieu for two specimens, which present two distinct varieties of this interesting stone.

One of them is of a good blue colour, is partly of a very fine sandy grain, and partly coarser and lamellated: it greatly resembles the carbonate of lime known by the name of saline marble. An immense number of small laminæ are observable in it, which cross each other in different directions, and are found by the magnifier to be perfectly rectangular.

The other is of a darker ash coloured grey; it is less pure than the former: some portions of a true gypseous earth are observed in it, containing a small quantity of argill and of carbonate of lime. Its substance is more compact than that of
the

the preceding specimen; its laminæ are larger, their rectangular form is more perceptible, and several of them have a pearly reflection.

Of two other pieces of the same substance, in the cabinet of ^{Other specimens.} Mr. Greville at London, which appear to me to belong to the anhydral sulphate of lime of the salt-works in the canton of Bern, and both which are perfectly pure, the one is white with a little of a blueish cast, the other has the same blueish tinge, but deeper, and greatly resembles in colour the pale blue sapphire, known by the name of water sapphire. Both have a coarse granulated texture, and are composed of a combination of rectangular laminæ, which cross each other in different directions, as may easily be discerned with the naked eye. But the laminæ of that in which the blueish colour is the most intense, present besides so lively a pearly reflection, that it might easily, at first sight, be mistaken for a mass of zeolite stilbite. Rectangular laminæ, of some thickness, may be separated from it, which, like the crystals before described, are easily divided in every direction.

The specific gravity of the anhydrous sulphate of lime, ^{Specific gravity.} mixed with common salt, proved to be 2940. That of the specimens which I mentioned in Mr. Greville's cabinet, namely, that in which the laminæ had the strong pearly reflection, was 2957, and the other 2929. Of the two of Vulpino, the most compact was 2951, and the other 2933.

The hardness of this substance, in all the specimens, is some- ^{Hardness.} what superior to that of the carbonate of lime; in all of them also the anhydrous sulphate of lime is fusible by the blow-pipe without ebullition, and affords an opaque glass.

There is a marked difference between them, with respect to ^{Phosphorescence.} the phosphorescence, upon an heated shovel. The anhydrous sulphate of lime of Vulpino, gives a pretty strong orange-coloured light; that mixed with common salt, affords a very faint bluish light; that of the salt-works of Bern, none at all; and lastly, that which I mentioned as containing actinote, with attractive oxide of iron, &c. gives a light somewhat more reddish than that of Vulpino.

Gypsum deprived of water, or anhydrous sulphate of lime, ^{The absence of water renders the properties altogether different.} therefore, possesses characters altogether different from those of the sulphate of lime which contains that liquid; and as in these two stones the sulphuric acid and the lime exist in the

IN THE ANHYDROUS SULPHATE OF LIME.

same proportions, it cannot be doubted but that the presence of the water totally changes the nature of the combination of acid and earth when it comes to be joined with it, which, it appears to me, can only take place so far as this water becomes itself an essential component part of the stone.

Remarks in proof that water is essential to the composition of many substances.

Arsenates of copper.

This is not the only substance which enables us to ascertain that the water, which formerly was considered as a part foreign to the stones which contained it, becomes in reality, in several of them, an ingredient essential to their nature. The analysis which Mr. Chenevix has made of the different species of arseniates of copper, have presented us a striking example of this, especially in that variety of the third species of my description (see Philosophical Transactions, 1801) to which I have given the name of Hematiform. A movement of decomposition, which is confined to the gradual loss of their constituent water, totally changes the colour of these arseniates, and at last completely discolours them, at the same time that it renders those that before had some feeble transparency, perfectly opaque. This loss of the water always commences at the exterior, and in this case the interior part preserves all its transparency as well as its colour, whilst the exterior part is discoloured, and exhibits by the shrinking of its surface, sometimes to a very considerable degree, sensible marks of the loss which it has sustained; the water amounting to about one-fifth of its mass*.

* In my description of the arseniates of copper quoted above, I have considered this hematiform arseniate, as well as those which I have designated by the terms of indeterminate, capillary, and amianthiform, only as being varieties of the species in the acute octahedral form: the copper and the arsenical acid are in fact contained in them in the same proportions; but the water, which adds a new constituent part to them, and which did not exist in those varieties of this third species, which are in perfectly determined crystals, forms with them a real hydrate of this third species. I therefore think that it would be proper to separate these varieties, in order to form with them a fifth species perfectly distinct from the third. The arseniate of copper is one of the most astonishing productions of the mineral kingdom, by the immensity of the aspects under which it presents itself, all which, nevertheless, have certainly a particular cause, which I am very far from pretending to have ascertained.

In

In the same manner it is, that, in the blue carbonate of copper, Blue carbonate of copper. which, according to the investigations of Mr. Proust, appears to contain a considerable quantity of water, far superior to that which exists in the green carbonate, the crystals seem to pass, at their surface, into the state of green carbonate, by the mere loss of a certain portion of their constituent water. This species of decomposition is sometimes even so considerable in them, that it exists, for example, amongst the specimens in large and superb crystals which come from Siberia, crystals, of which the form having undergone no change, belongs to the blue carbonate, but which have entirely passed, throughout their whole substance, into the state of green carbonate.

I am fully persuaded, that when water is once considered and admitted as a constituent part in the composition of numerous mineral substances, it will soon be acknowledged to contribute greatly, by its presence or its absence, to the difference which subsists between several stones: thus I am strongly inclined to believe that the carbonate of lime of slow solution, the hardness and specific gravity of which are so much superior to the same characters in the ordinary carbonate of lime, differs from this latter perhaps only by a difference in the water of composition. Most certainly this difference cannot proceed either from the presence of argil, or from that of magnesia. I know some dolomies which certainly do not contain any trace of argil, whilst, at the same time, I know carbonates of lime, which are highly charged with magnesia, and which, nevertheless, are very speedily dissolved in the acids: Of the number of these latter are, for example, most of the calcareous spars with a pearly reflection and greasy aspect.

I am much inclined also to think, that the species of Chalcedony, named cacholong, may owe its difference from the common chalcedony only to the circumstance, that the latter contains water of composition of which the cacholong is destitute; Water is of great importance as an ingredient which modifies the properties of bodies. and that the transition of the chalcedony into this state, and subsequently into the hydrophanes, depends, in a great measure, upon the loss of this water. The probability of this opinion remains to be settled by future observations.

IX.

Analysis of Natural and Artificial Anhydrous Sulphate of Lime, by
 RICH. CHENEVIX, Esq. F. R. S. M. R. I. A. *Communicated by the Author.*

Component parts
 of common sul-
 phate of lime.

THE proportion of the elements in common sulphate of lime, such as I have stated them in the Transactions of the Royal Irish Academy, are somewhat different from those given by former chemists. Monf. Fourcroy, in his "*Système des Connoissances Chimiques*," and in his "*Synoptic Tables*," has determined them in the following order: sulphuric acid 45, lime 32, water 22. If we deduct the quantity of water, and reduce the remainder to the quintal, we shall have the following proportions: sulphuric acid 58,5, lime 41,5. As the object of the present Paper is merely the Anhydrous sulphate, I shall not take further notice of the quantity of water that may be contained in common sulphate of lime, whether natural or artificial.

Lime dissolved in
 muriatic acid
 and sulphuric
 acid added, and
 the whole ex-
 posed to violent
 ignition, left dry
 sulphate of lime
 containing 56,3
 lime, and 43,6
 acid.

I took one hundred parts of lime, prepared with the greatest care, and dissolved them in muriatic acid. I then poured sulphuric acid into the solution, and heated the whole in a platina crucible, at first gently, but afterwards to violent ignition. The augmentation of weight in the lime and the crucible, (which had been weighed before the operation) amounted to 78,5, and was combined sulphuric acid. This experiment therefore indicates, that strongly calcined sulphate of lime is composed of lime 56,3, and 43,6 of sulphuric acid.

Sulphate of ba-
 rytes contains
 24 acid in the
 100 parts.

I then took one hundred parts of calcined sulphate of lime, and decomposed them by oxalic acid, to render them more soluble, and then dissolved them in muriatic acid.

I precipitated the solution by muriate of barytes, and obtained 182 of sulphate of barytes. Hence it is evident that 182 of sulphate of barytes, and 100 of calcined sulphate of lime contain the same quantity of sulphuric acid = 43,6; which proportion gives 24 as the quantity of sulphuric acid, in 100 of sulphate of barytes. This quantity is nearly intermediate between that given by Thenard, and that which I had already stated. By this experiment I established a standard, to which I might refer every kind of sulphate of lime.

It is evident that calcined sulphate of lime, or artificial anhydrous sulphate of lime, contains 43.6 of sulphuric acid. It remains to prove its chemical identity, with the natural anhydrous sulphate.

I took one hundred parts of this substance, in as pure a state as the Count de Bournon could procure it, and submitted it to the same experiments. I obtained from this experiment 187 of sulphate of barytes, which announces 44.88. I shall never expect more uniform results in two analyses, even of the same substance, and do not hesitate to pronounce, that the two sulphates are, chemically speaking, one and the same thing.

The natural anhydrous sulphate of lime was treated like the artificial, and gave 44.88 acid.

The Abbé Hauy in his late work has given the proportions in anhydrous sulphate of lime, according to an analysis of Monsr. Vauquelin. This excellent chemist found its elements to be nearly in the inverse order of my statement, and such as Fourcroy has established them for the artificial sulphate, deducting the water of crystallization. I should still have doubted the accuracy of my experiments, if I had not discovered a cause that may explain the difference which exists between his results and mine.

Vauquelin's experiments gave much more acid;

The French chemists have mentioned two varieties of sulphate of barytes, one of which contains 13 per cent. of sulphuric acid, and the other 33. If therefore we estimate the quantity of acid contained in sulphate of lime, by the sulphate of barytes containing 33, and not by that containing 24 per cent. we shall have a much greater quantity of sulphuric acid, as a constituent part of sulphate of lime.

most probably from his assuming a higher proportion of acid in the sulphate of barytes.

Among the specimens which the Count de Bournon gave me for trial, there were some which contained muriate of soda. This salt was easily extracted by water alone; and the proportion of it differed in different specimens. Klaproth had some specimens in which he found carbonate of lime, and even filica. But as I have examined some in which I can positively assert that there was neither the one nor the other, those substances may be looked upon as merely accidental.

Some specimens of the anhydrous sulphate contained muriate of soda;

but it was accidental.

X.

Abridgment of a Memoir of Mr. PROUST on Tanin and its Species.*

Uncertainty of the process of separating tanin by muriate of tin.

THE process which Mr. Proust has given for separating Tanin by muriate of tin is subject to considerable uncertainty when it is used, as an English Chemist has lately done to fix the proportion of this principle in vegetable juices. He thinks, therefore, that it may be of utility to announce these causes of error, in order that philosophers, who are engaged in this department of research, may place less confidence in it, and turn their thoughts to some more perfect method. To these remarks he adds an account of certain varieties which he thinks he has perceived in the genus of Tanin.

When the oxide of tin is seized by the tanin, the muriatic acid instead of being set at liberty dissolves part of the tannate.

1. When this muriate is saturated with an astringent juice, it happens that the muriatic acid takes up in solution a portion of the tannate of tin, so that what is collected on the filter represents, in truth, only a part of the tanin principle contained in the plant. This effect is similar to what happens in the preparation of ink, the black dye, and in every case where a dyeing principle can deprive an acid of the oxide which it held in solution. The affinity of this acid not being capable of remaining inactive exerts itself upon the coloured oxide.

This tannate may be thrown down by careful addition of alkali.

When small doses of alkali are added to the liquor the residue of the tannate may be made to precipitate without even touching the gallic acid if it be present; and if this point be exceeded it may soon be perceived by the green colour which the fluid receives from contact of the air. In that case a few drops of acid will be sufficient to dispel the cloud by seizing the excess of alkali and setting the gallic acid at liberty. But as, on the other part, it is necessary at first to employ an excess of muriate to assure the precipitation of the whole of the tanin, there is danger of loading the tannate with a certain excess of oxide of tin.

But the tannate will be overcharged with oxide if too much of the muriate has been used.

The muriate precipitates extractive matter and perhaps other principles along with the tanin,

2. The presence of tanin does not exclude the extractive principle in the juice of a plant, and as the muriate precipitates this last as well as the former, the extractive principle will therefore become a new source of mistake in the estimate of

* From the *Annales de Chemie* XLII, 89.

the quantity of tanin. It may also happen that the vegetable juices may contain many other substances capable of decomposing the muriate either directly or indirectly, so that this re-agent can never be depended upon with confidence.

There are besides earthy salts in these juices as Vauquelin has shewn. Some, as for example Sumac contain sulphate of lime in abundance. If therefore we use the alkali to complete the separation of the tannate, the precipitate will also be charged with an earthy deposition.

When earthy salts exist in the vegetable, these will let fall their bases when the alkali is added.

3. On reflecting upon the means of avoiding the errors caused by the muriatic acid, Mr. Proust tried a process which he had successfully used to separate the colouring principle from the gelatinous mucilage which accompanies it in cochineal. It consists in heating, or even simply agitating, the astringent juice with the oxide of tin prepared by nitric acid, and kept under water. The oxide in fact, becomes loaded with tanin in a few days. But if the juice of a plant which is not astringent, or a diluted extract, be treated in the same manner, it will also become deprived of the whole of its extractive matter: and the gum and the salt will remain alone in the fluid. This method cannot therefore lead us to our object.

If the pure maximum oxide of tin be heated or agitated with tanin a combination takes place and, mucilage, if present, is left:

but this process will not separate the extractive matter.

4. What appears most surprising to this chemist in the present experiment is the destruction of the gallic acid, or probably its transition into a state in which it cannot perform the functions of that acid. The fluid in fact when cleared of the tannated oxide by the filter has no longer either colour or taste, and makes not the slightest impression on the solutions of iron, nor even upon turnsol. When examined by every trial the fluid is found to be mere water.

Singular event in the last experiment. The gallic acid is destroyed.

5. Suspecting, nevertheless, that this acid might be combined with the oxide of tin, he passed the tannate of the last described process into potash. The product was a coloured fluid, in which he found no sign of the gallic acid: for it did not assume by exposure to the air that green shade which it always indicates when saturated with an alkali. He precipitated the tanin by a diluted acid, a portion remaining in solution, as happens in this case and he proceeded to examine that which remained upon the filter; but he soon perceived that it had also advanced towards a state in which, as we shall shew, he found infinitely less tanin than before.

When the tannate &c. was decomposed by potash no gallic acid was found and much less of tanin.

The tanin that is separated is not totally soluble in boiling water; neither does it precipitate glue, nor has it the taste or smell of tanin &c. It resembles extracts.

6. Boiling water cannot totally dissolve it. Its solution no longer precipitates glue; it has neither the harsh taste nor the odour of tanin. With the red sulphate of iron it affords only a whitish grey precipitate, and lastly it does not afford a magma with the muriate of tin; it is precipitated merely in the manner of extracts, to the taste of which it in some measure approaches though it does not possess their bitterness. These are the alterations to which the tanning principle is subject when combined as before mentioned with tin oxidized to the maximum.

It was suspected that the tanin had taken oxygen from the oxide; but experiments on the latter did not seem to indicate this loss.

7. From these changes he suspected that the tin might have yielded to it that portion of oxygen which constitutes the difference between the oxide at the maximum and the oxide at the minimum, as happens with the oxide of iron in ink, hermetically closed. In order to ascertain this he dissolved in muriatic acid the oxide what had been deprived of tanin by potash; but he discovered no indication of that kind. The solution produced no change in that of gold, nor in corrosive sublimate. It was at the maximum. It is true that with respect to the oxide of tin at the minimum, as well as that of iron, washing and exposing to the air speedily bring it to the maximum.

Theory of this conversion yet unknown.

Whether by oxidation or by whatever other process, the tanin principle did at length pass to the state of ordinary extract. Tanin precipitates glue, but extract does not; this is the difference between them. The influence of some affinity, which the author has not sufficiently developed, must have changed its radicals in their primordial arrangements or in their proportions; and it may be supposed that the gallic acid likewise after having been subjected to these changes became assimilated in the same state and by the same causes.

Concerning the Varieties of Tanin.

The genus *tanin* composed of species.

If in the series of immediate principles which compose the whole of vegetable matter we consider the tanin of galls as a genus, because this in fact possesses the qualities in the highest degree, it is easy to form a notion that this genus may have its species, and may, as well as sugar, gum, starch, &c. affect different modifications. There are various species of sugar, resin, gum &c, and there may be also various species of tanin. Mr. Proust thinks that he has found this to be the case.

Cachou

Cachou, or Terra japonica.

Cachou is an astringent: it is soluble in alcohol and ^{Qualities of the various species of tanin.} in water. It precipitates glue abundantly and forms with it a vinous (q.) magma which has neither the consistence nor the insolubility of the tannate of galls. ^{Cachou or Terra japonica.}

It reduces muriate of gold, it precipitates the muriate of tin and affords a violet coloured ink with red sulphate of iron. It is a tanin *sui generis*. It dyes silk.

Dragons Blood.

That which is pure and comes to us in calabashes is soluble ^{Dragon's Blood.} both in water and in alcohol; its taste is harsh; it dyes silk of a bad wine colour. It abundantly precipitates glue, the muriate of tin, the red sulphate, and difoxides gold; it is also a species of tanin.

Sumach.

The tanin which this substance contains, abundantly pre-^{Sumach.} cipitates glue, and affords a white magma without consistence. Like tanin it is separated from the decoction of sumach by the carbonate of potash; its curdled deposition is again soluble in hot water with the exception of a small quantity of chalk.

Barites and the oxalic acid demonstrate the existence of abundance of lime and sulphuric acid; but it will be necessary to ascertain by experiments on the green sumach whether sulphate of lime be one of its principles, or an adulteration. The author is not surprised at finding it, since he obtained this salt in considerable quantity from the juice of cabbage, and of *solanum lycopersicon*, which is cultivated in the gardens under the name of *lomates*. This juice also contains gallic acid; it becomes green in the air when saturated with potash. It reduces gold, decomposes muriate of tin and the red sulphate, with which it affords a thick ink.

Yellow Wood, or Fustic.

It contains a species of tanin. Like tanin it precipitates ^{Yellow wood or Fustic.} the solution of glue; a solution of salt is sufficient to precipitate it. It is soluble in water and in alcohol. It reduces gold, decomposes muriate of tin and the red sulphate, by means of which it dyes silk of a greyish yellow.

Fiset

*Fusci.**Fusci.*

It is a pure dyeing extract soluble in water as well as in alcohol. It contains a small quantity of gallic acid, but does not change the solution of glue. It reduces gold, precipitates the metallic salts, and has no gummy portion.

*Grains of Avignon, or French Berries.**Gr. d'Avignon, or French berries.*

They afford a dyeing extract of the same nature, without gum or tanin. It reduces gold, &c.

*Brazil Wood,**Brazil wood.*

Affords also a dyeing extract soluble in alcohol, without tanin, or gum, reducing gold and precipitating the metallic salts.

Reduction of gold is no exclusive character of tanin.

Mr. Proust has remarked, that the reduction of gold has ceased to be a characteristic quality, since he observed the muriatic solution abandon this metal to all the tinging substances, such as anise, cochineal, gum guttae, gallic acid, verjuice, wine, vinegar, the juice of all fruits, manna, gum, and sugar, though somewhat slowly.

Concluding observations.

The author concludes by observing, that tanin has its varieties like the other immediate products; that the property of precipitating glue is the generic indication, by which they are distinguished from extracts, which do not alter that substance; and lastly, that the different species of tanin, particularly those which have been discovered in the barks of trees, cannot be compared together as to their force and their useful qualities, but by observations upon skins which have been submitted to their action.

*Tanning matters require to be tried by actual preparation of leather.**Sulphate of lime very common in vegetables.*

Note. Plaster must be infinitely common in vegetables. Mr. Proust has found it in verjuice, grapes, apples, gooseberries, &c.

B. L.

XI.

Description of an Apparatus for heating Water by waste Steam.

Invented by Mr. ARTHUR WOOLF.

THE following apparatus was erected at the extensive Engine for heating water by steam. brewery of Messrs. Meux and Co. in August, 1800, and has been in use ever since. I saw it work a few weeks ago, and observed with great pleasure the facility and precision with which it operates, and I have great satisfaction in presenting it to the reader as a very judicious and useful combination.

Plate IX. A represents a steam pipe from the brewing Particular description. copper.

B a valve with its weight.

C the vessel in which the steam is condensed.

D a pipe that conveys the cold water from a reservoir.

E a conical valve through which the water is injected. It is connected with the lever F.

G is a bended pipe to prevent any of the steam from escaping with the hot water.

H a small receiver from which the hot water may be conveyed to different situations by means of pipes and cocks.

I a pipe open to the receiver to prevent a vacuum in case the water should be made to descend in any of the pipes.

K a small pipe to convey the steam into the regulator.

L the regulator which is composed of three cylinders, the outside and inside being closed together at bottom, leaving a cavity between, which is filled with water; the middle or moving cylinder is inverted and close at top. It serves for a piston, and is connected to the lever M, on which is a sliding weight N, by which the quantity and heat of the water may be varied at pleasure.

O is a valve through which the steam is let out when not used for heating water.

The effect of this engine may be easily understood. The Explanation of the manner in which it acts. weight of the inverted hollow piston L presses down the valve E by means of the levers, and this pressure may be regulated by fixing the weight N nearer or farther from the centre of the upper lever. When the steam through A has acquired a certain degree of strength in the vessel C, it raises the piston by its action through K, and consequently opens the valve E. A sheet of water immediately dashes through, as represented

in the figure, and by condensing the steam, suffers L again to descend; and, after a vibration or two, the effect of the steam to raise the piston and of the injection to depress it, balance each other, so that the levers remain nearly motionless. It is evident that the injection will be less, the steam stronger, and the water which passes off through G hotter the nearer the weight M is to the centre of motion. And in this respect the apparatus is so effectual that the water may be heated to 210 degrees, and the quantity that passes off is from 100 to 180 barrels per hour, according to the temperature, as governed by the position of the weight M.

XII.

Description of an improved Drawback Lock for House Doors. By Mr. WM. BULLOCK. From the Transactions of the Society of Arts, who adjudged a Reward of Fifteen Guineas to the Inventor.

Copy of a Letter to the Secretary of the Society Mr. CHARLES TAYLOR.

S I R,

Inconveniences
and danger of
the common
drawback lock.

I HAVE herewith sent, for the inspection of the Society, an improved Drawback Lock for House Doors, &c. which improvement is in latching the door; for it is well known, particularly in damp weather, that the air drawing through it, rusts the head or bevel of the bolt, by which means it requires great force to shut the door, and occasions a disagreeable noise, besides shaking the building.

It has frequently happened that the house has been exposed to robbery from the door being left unlatched, when supposed to be fast. This improvement removes all those inconveniences, as it lets the bolt shoot into the staple immediately when the door closes, but not before; and the reliever works so very easy, that the door is made fast with one twenty-fourth part of the force required with locks upon the common construction.

By

By an experiment with the lock sent herewith, it will be proved that two ounces added to the reliever, will shoot the lock with more ease than three pounds will do, applied to the bevel bolt; and if the lock is rusty, the advantage will be much more in favour of the new method. I flatter myself it will be of great utility to the public, as its construction is simple and cheap. It may be added to any old lock, as may be seen from that now sent. It may be advantageously applied to French windows and glass doors, as it prevents the door from being strained, or the glass broke, by the force applied to shut them. I have fixed several locks, upon this new principle, which answer well; and if the invention meets with the approbation of the Society, I hope to be rewarded according to its merit.

Advantages of
the improved
lock.

I remain, with respect,

SIR,

Your most obedient Servant,

WILLIAM BULLOCK.

No. 6, Portland Street, Soho, May 5, 1801.

Plate X. Fig. 2. A. Is the new iron latch here affixed to an old common drawback house lock.

B. An iron pin at one end of the latch, on which pin it is moveable.

C. A projecting part of the latch, which, when the common spring bolt D of the lock is drawn back, in the usual manner, is forced into the nick on its higher part at E, by the spring F, underneath the latch.

The bolt D then remains within the lock, until, on closing the door, the reliever G gently presses on the lock box, fixed in the common way on the door cheek; which pressure draws the projecting part C out of the nick E, and permits the end of the bolt D, by the force of the spring G, to slide into the lock box, and fasten the door.

XIII.

*Description of an improved Mill for grinding hard Substances.
By Mr. GARNETT TERRY. From the Transactions of the
Society of Arts, who adjudged the Silver Medal to the Inventor.*

Description of
an improved
mill.

MR. Terry, whose residence is No. 29, City Road, Finsbury Square, has constructed this mill on a large scale, and there is also a model deposited in the Society's collection.

Plate X. Fig. 1. A. The hopper, or receptacle of the articles which are intended to be ground.

B. A spiral wire, in the form of a reversed cone, to regulate the delivery of them.

C. An inclined iron plate, hung upon a pin on its higher end: the lower end rests on the grooved axis D, and agitates the wire B.

D. The grooved axis, or grinding cylinder, which acts against the channelled iron plate E.

F. A screw on the side of the mill, by means of which the iron plate E is brought nearer to or removed further from the axis D, according as the article is wanted finer or coarser.

G. The handle, by which motion is given to the axis.

H. The tube from whence the articles, when ground, are received.

* * * The front of the mill is taken off, in order to show its interior construction.

XIV.

*Remarks on Dr. Thomson's Theory of Combustion. By C. R.
(Received June 15, 1802.)*

General remarks
on Dr. Thom-
son's theory,
and the distinc-
tion between
combustion and
oxygenation.

THE scientific world are so highly indebted to Dr. Thomson for many original communications, and for the very perspicuous manner in which he has explained many of the phenomena of chemistry, that every thing that is presented to the world under the sanction of his name, is intitled to much consideration: if in some instances we are induced to hesitate in the yielding of our assent, we cannot but do justice to the ingenuity of his reasonings, and at the same time acknowledge the

the very luminous manner in which he conveys information, on every subject that he treats. The Paper under consideration is particularly an instance in point, and if we cannot go the full length with the author, we must at least acknowledge, that in the chief, his distinctions are accurate, and his reasoning just. Nothing can be more evident than the difference which in numberless instances prevails, between the act of oxygenation in bodies, and that of combustion, inasmuch as neither the phenomena attending them, nor the results arising therefrom, are the same. The French chemists, however, seem to have been misled, in their confining the term combustion to the act of oxygenation, by considering, that all bodies during their combustion combine with oxygen, without at the same time recollecting, that this latter effect may be produced without any of the phenomena usually attendant on combustion, and that though certainly all combustion presupposes the combination of oxygen with a base, yet this combination may, and repeatedly is effected where no combustion can possibly take place.

That a distinction therefore prevails between the two is obvious, and the Doctor offers us a theory, which he considers as sufficient to explain the different phenomena produced.—This theory it is the purpose of the following lines to shew however ingenious, and apparently satisfactory it may appear to be, is not wholly adequate to the task that is assigned to it.

It will be necessary very shortly to state here the outline of the theory under consideration. In all cases says Dr. Thomson *, when heat and light are extricated during combustion, it will be found, that the light is furnished by the combustible or burning body, and the heat by the decomposition of the oxygen, which forms a component part of the supporter, and which is essential to the combustion, and that the distinction that prevails between the two processes of combustion, and of oxygenation, arises from the difference of the phenomena, which accompany the action of supporters and products upon other combustibles. “The supporters convert these bodies into products, and combustion, or the emission of heat and light at the same time take place; whereas, the products convert combustibles into products, without any such emission.

Outline of the theory. That the light of combustion is furnished by the combustible body and the heat by the oxygen of supporters;

but that products convert combustibles into products by mere oxygenation without combustion.

* Philof. Journal, New Series, II. 10 and 92.

Now as the ultimate change produced on combustibles by both these sets of bodies is the same, and as the substance which combines with the combustible is the same, namely oxygen, it is evident, that the oxygen of the supporters contains something, which the oxygen of the products wants," and this something the Doctor supposes to be caloric. "In the same manner the combustibles and products resemble each other, the chief difference between them consisting, in the phenomena which accompany their combination with oxygen, in the one case fire is emitted, and in the other not." Now says the Doctor, "if we recollect, that no substance but a combustible is capable of restoring combustion to the base of a product, and that at the time of its doing so, it always loses its own combustibility, and further, that the base of a product does not exhibit the phenomena of combustion even when it combines with oxygen, we cannot avoid concluding that all combustibles contain an ingredient, which they lose when converted into products, and that this loss contributes to the fire, which makes its appearance during the conversion." This ingre-

Leading positions of the theory, 1. that light is originally an ingredient of combustible, 2. and heat of oxygen.

Many reasons why the heat must come from the oxygen of the supporter.

Difficulties as to the other position, that the light invariably

dient the Doctor supposes to be light. It is evident, that the two leading positions of this theory are, 1. That during combustion, all combustibles emit light, which previously formed a necessary ingredient to their own composition; and secondly, That the heat which is evolved during the process of combustion, proceeds from the oxygen of the supporter, of which it likewise originally formed an essential ingredient.—That the heat given out during combustion comes from the decomposition of the oxygen of the supporter, there are many reasons for concluding. We know very well that no combustion will take place without the presence of oxygen, and that the greater the quantity of oxygen absorbed in a given time, the greater is always the heat that is evolved. Now if the heat be not supposed to come from the oxygen, why should the degree of heat given out, be always proportional to the quantity of oxygen that is absorbed, and upon what other principle can we so satisfactorily explain the effects that are produced by the Argand lamp. These considerations, combined with the argument drawn from the maintenance of the temperature of hot blooded animals by the decomposition of air, seem sufficiently to establish the truth of the foregoing position. There are, however, many difficulties that press against our implicit adoption of the other

other part of this theory, viz. that the light emitted during combustion invariably proceeds from the burning body, and that consequently it forms no part of the supporter. It of course then follows from this theory, that light is no essential part in the composition of oxygen gas. Let us however see whether this be the case. Many facts it will be found concur to prove that the contrary is the truth. If nitric acid be exposed to the light, after some time we find that it changes colour, it becomes yellow, green, and then red, and oxygen gas is disengaged, the nitric at the same time being converted into nitrous acid. Now it is evident, that as this decomposition is of a chemical nature, the light that occasions it, either combines with the oxygen to form oxygen gas, or with the acid to form nitrous acid: as we find no dissimilarity between the nitrous acid procured by this means, or that by any other, we are necessitated to conclude that the light has combined with the oxygen, and that the latter by the same means is converted into oxygen gas. Again, it is well known, if oxygenated muriatic acid be exposed to the rays of the sun in a transparent bottle, there is disengaged from it oxygen gas; in proportion as the gas is separated the acid loses its colour and odour, and returns to the state of simple muriatic acid.—Here it is evident, that the oxygen has passed from a concrete into a gaseous state from the combination of light, and we must therefore conclude that light is a component part of oxygen gas.

If phosphorus be inserted in nitric acid, the latter is decomposed, and a product of combustion, namely phosphoric acid is formed, during which process neither heat nor light are given out. This process Dr. Thomson considers as an act of oxygenation, and not of combustion, because, says he, though a product of combustion is formed, a new supporter, namely nitrous gas is evolved, and the formation of a combustible, or new supporter, constitutes one of the characteristic differences between the two processes of combustion and oxygenation. Now it is said, that in all cases of oxygenation a double decomposition takes place, the oxygen of the product combines with the base of the combustible, while the light of the combustible combines with the base of the product. The question then naturally presents itself,—what during this process becomes of the light which made a component part of the

proceeds from
the burning
body.

has been
disengaged

For the light
which decom-
poses nitric or
ox. mur. acid is
concluded to
have combined
with the disen-
gaged oxygen.

When phospho-
rus is acidified in
nitrous acid,
what becomes of
the light of the
combustible?

phosphorus previous to its conversion into a product? It cannot combine with the new supporter that is evolved, because it is a part of this theory, that light is no constituent part of supporters, but only of combustibles; it should therefore have been made evident to the senses, which we do not ever find to be the case, nor is any heat evolved; this latter effect is no doubt very easily explained, but what becomes of the light yet remains to be shewn.

Sulphurous and
sulphuric acids.

Sulphuric acid, says Dr. Thomson, is a substance which from many of its properties I conclude to be a combustible, and not a product. This conclusion, however, does not appear to be perfectly consistent with the definition the Doctor has given of combustion, for when sulphur is heated in the air to the temperature of 302 degrees, it gives out light and heat, and is converted into an acid, *viz.* sulphurous acid: this according to the theory under consideration is a complete act of combustion, and therefore only a product of combustion, and not a combustible body ought to be formed. Sulphurous acid, according to La Grange, combines slowly with oxygen, and is converted into sulphuric acid, but as no light and heat are rendered visible, ought it not in this case rather to be considered as an act of oxygenation; for if light and heat were evolved in this process, it should appear that combustibles are capable of giving out a part only of their light in some cases, and the whole in others, which does not appear very probable, for it cannot be doubted but that in sulphurous acid, the oxygen and the sulphur mutually saturate each other, and that sulphuric acid is only sulphurous acid combined with an additional dose of oxygen. Though the Doctor apparently reconciles the decomposition of water by iron or zinc with his theory, it yet appears to be attended with some difficulties which are not easily explained.—“Whenever, says he, the whole of the oxygen is abstracted from products, the combustibility of their base is restored as completely as before combustion, but no substance is capable of abstracting the whole of the oxygen from such products, except a combustible, or partial combustible. Water, for instance, is a product of combustion whose base is hydrogen; to restore the combustibility of the hydrogen, we have only to mix water with iron or zinc filings, when the metal is oxidated, and the hydrogen gas is evolved as combustible as ever.” Let us here attend to the phenomena

Decomposition
of water by iron
or zinc.

which

which should take place according to the Doctor's theory; water, which is a product of combustion, is hydrogen without its light, in union with oxygen, without its heat; by adding iron (a combustible containing light) we decompose the water, that is to say, 73 parts of iron unite with 27 of oxygen. Now as to every 27 parts of oxygen in water there are about four of hydrogen, of course these four parts of hydrogen are liberated; but as it does not appear probable that combustibles should be capable of combining with light in all proportions, it may be asked, if the 73 parts of iron which are oxidated contain just light enough, and no more, to restore the combustibility of the four parts of hydrogen; for if there be too much for that purpose, the superabundant quantity ought to become visible, and if too little, a part only of the hydrogen should recover its combustibility; and be converted into gas. And the same reasoning may of course be urged with regard to the decomposition of water by zinc: for it cannot but be acknowledged, that the fact is somewhat singular, that the product of combustion should always contain and give out the precise quantity of light which is sufficient to restore combustibility to the base of the product, and in no case either more or less. Thus it appears that there are many difficulties that attend our implicit assent to the present theory, and many of the phenomena of combustion that do not apparently admit from it of an easy interpretation. Whether Dr Thomson can reconcile these apparent anomalies to it, remains to be seen; but if it is found equal to their solution, there could then it should seem be little objection to its adoption. At all events, no one will be inclined to dispute, that the Doctor has thrown much light on a subject, which before its investigation by him, was considerably more obscure: and that he has placed it in a new point of view, which bids fair to enable us to approximate much nearer to a true theory with regard to the phenomena of combustion, than any other that has hitherto prevailed.

Numerically considered as to the transition of precisely adequate portions of light from combustible to combustible, &c.

Conclusion.

Dr. T. has thrown great light on this obscure subject.

C. P.

IN CERTAIN POINTS OF NOMENCLATURE.

XV.

On certain Points of Nomenclature. By a Correspondent.

To Mr. NICHOLSON.

SIR,

On the use of *y*
in English for
the Greek
vowel *υ*.

APPREHENDING that your mode of writing certain newly imposed names of substances in chemistry, arises from inattention, and being misled by the French, I take the liberty of a friend to remind you, that, in English, it is not usual to write the vowel *i* for the *υ* of the Greeks, but *y*; hence, in our language, we do not write oxygen, hidrogen, oxygenised, &c. but oxygen, hydrogen, oxygenised, &c. : you write, however, properly, oxide instead of oxyd, as some persons spell the word; because oxide shews the etymology in *οξυς* and *υδο*; better than oxide.

Your's ever faithfully,

A. B. C.

ANSWER.

I DO not profess to have directed much attention to the subject of nomenclature; though I am well aware of its importance to the acquisition, as well as the communication of knowledge. It is, therefore, with considerable diffidence that I state my apprehension, that neither usage in a language, nor the motive of precisely indicating the derivation of a term, are very cogent arguments for adopting any particular mode of structure, if other motives present themselves. To me it seemed at least as forcible a reason for the use of *i* instead of *y*, in the words alluded to, that, together with their derivatives, they are so very numerous, as to make it desirable to accommodate them to the general usage of the modern languages; and this appeared to be promoted by following the change proposed by the framers of the chemical nomenclature.

XVI.

Duplicate Copy of a Letter from BARON DE ZACH to the Right Honourable Sir JOSEPH BANKS, Bart. P. R. S. &c. transmitted to Mr. EDWARD TROUGHTON, and communicated by the Rev. J. PEARSON; on the new Planets Ceres and Pallas, with the Elements of the Orbit of the former.

Seeberg Observatory, near Gotha, May 31, 1802.

MOST HONOURED SIR,

HAVING prosecuted Dr. Olber's *Pallas* from April 4 till May 11, in the meridian, Dr. Gauss, upon this set of my observations, calculated the elements of an elliptical orbit of this very remarkable heavenly body, which represent, with great accuracy, all the Seeberg observations.

It appears, in general, by these calculations, that *Pallas* is a planetary heavenly body, that moves between the orbits of Mars and Jupiter, with a very great eccentricity and inclination, and whose orbit comes very near to the orbit of the planet Ceres, perhaps touches it, perhaps even cuts it, like two links of a chain, this way ∞ , which cannot yet be asserted with certainty, the observed arc run over by this planet being too small. Notwithstanding, it appears already that the distances of *Pallas* and Ceres, in the line of nodes of their orbits, is very nearly equal. In the descending node, the distance of *Pallas* from the Sun is = 2,86, and the same distance of Ceres = 2,93. In the ascending node, these distances are of greater inequality. Another very remarkable circumstance is, that the mean motions of *Pallas* and Ceres are very nearly, perhaps absolutely the same; though this cannot yet be asserted, because the error of the observations of both planets is still too great. But as far as yet appears, these mean motions will not differ very much; and in this case, small as the masses of Ceres and *Pallas* may be, they will nevertheless exert a very sensible action one upon the other, and therefore give occasion to very curious and interesting investigations in the mechanics of the heavens. The new planet *Pallas* will also call forth the utmost exertion of our analytical powers. Hitherto the two elements of a planetary orbit, viz. the eccentricity and the inclination, had been considered as an infinite little quantity, and so it might be, as these

Orbit of *Pallas* very eccentric, and probably linked in that of Ceres.

Interesting remarks on the singularity of the orbit of *Pallas*.

THE NEW PLANETS CERES AND PALLAS.

o elements, in all our old planets, are very small, so that the higher powers of them could be neglected without danger, in calculating their mutual action, as they produced no sensible term in the approximating series. But this is no longer the case with Pallas, in which the eccentricity of the orbit and the inclination are so very great.

Elements of
Pallas.

*Elements of the Orbit of the Planet Pallas, calculated by
Dr. Gauss.*

Epocha, March 31, at noon, in Seeberg,	161° 12' 43,"2
Aphelium	300 5 4, 0
Node	172 34 35, 0
Inclination	35 0 42, 0
Mean daily heliocentric and tropical motion	757" 166
Logarithm of half the greater axis	0,4472636
Eccentricity	0,2591096

By these elements the whole series of my observations are

represented,

TABLE.

Computed and
observed places.

Pos.	R.A. by Calculation.	Difference.	Decl. by Calculation.	Difference.
Apr. 4	183° 44' 5,"7	— 0,"9	13° 54' 55,"4	+ 3,"4
5	183 34 24, 6	+ 0, 9	14 13 22, 6	— 0, 3
7	183 15 39, 6	+ 1, 6	14 49 1, 3	— 0, 7
8	183 6 35, 4	— 2, 4	15 6 15, 2	+ 5, 2
15	182 10 17, 9	+ 1, 4	16 54 35, 7	+ 4, 9
18	181 50 29, 4	— 1, 2	17 34 27, 1	—
19	181 44 30, 7	+ 5, 4	17 46 51, 9	— 2, 5
24	181 19 35, 8	— 2, 0	18 42 27, 9	—
25	181 15 38, 3	+ 6, 1	18 52 18, 9	—
26	181 12 2, 0	+ 0, 2	19 1 44, 9	— 4, 5
27	181 8 46, 6	+ 1, 0	19 10 46, 3	— 0, 5
29	181 3 21, 2	+ 4, 6	19 27 36, 2	— 7, 5
30	181 1 11, 4	+ 1, 5	19 35 24, 9	— 8, 6
May 1	180 59 22, 9	+ 0, 4	19 42 50, 8	+ 10, 0
2	180 57 56, 7	+ 4, 8	19 49 53, 2	+ 1, 6
3	180 56 52, 4	— 3, 2	19 56 32, 9	+ 14, 8
5	180 55 49, 6	— 5, 0	20 8 45, 6	+ 6, 9
6	180 55 51, 4	— 2, 7	20 14 19, 4	—
7	180 56 14, 9	— 8, 9	20 19 32, 1	— 3, 2
8	180 57 0, 4	— 9, 6	20 24 23, 7	+ 3, 6
11	181 1 25, 5	— 16, 2	20 36 58, 8	— 14, 4

The differences applied with contrary sign to the calculated
A. R. and Declin. will give the observed A. R. and Decl.

Pallas

Pallas and Ceres are now too near to the sun, and the twilight permits no meridian observations. But astronomers who are provided with equatorials of great perfection, as, for instance, those of Greenwich, Oxford, Richmond, and of Sir George Shuckburg, will be able to follow these two planets a longer time. The observation of Pallas will chiefly be of a very great value, as the series of meridian observations is not for above five weeks. If more observations are not procured, it will be with some difficulty we shall find Pallas again next year; for the elements of an orbit calculated upon so small an arc as $7\frac{1}{2}^{\circ}$, may give an error of several degrees in January 1803. You will do, most honoured Sir, a great benefit to science in general, and to astronomy in particular, if you engage the English astronomers, who have so very excellent and fixed equatorial sectors, to follow Pallas out of the meridian as far as they can. For this purpose, I take the liberty to send you here an ephemeris of this planet's motion, calculated by Mr. Gauss, which will enable astronomers to find it, and pursue their observations.

These planets
are now in the
twilight.

Ephemeris of the Position of Pallas for Midnight, in Seeberg Observatory.

Ephemeris of
Pallas.

1802.	R. Ascens.	Declin.
May 24	181° 57'	21° 1' N.
27	182 18	21 0
30	182 41	20 57
June 2	183 6	20 52
5	183 34	20 46
8	184 5	20 38
11	184 57	20 28
14	185 12	20 17
17	185 49	20 5
20	186 27	19 52
23	187 7	19 37
26	187 49	19 22
29	188 32	19 6

I am, with the greatest esteem and regard,

MOST HONOURED SIR,

Your most humble and
obedient servant,

FRANCIS BARON DE ZACH.

XVII. Method

XVII.

*of applying a temporary Forcer to a Pump, so as to produce a constant Stream. By Mr. RICHARD TREVITHICK *, From the Author.*

THIS contrivance which, on several occasions, may prove useful, consists in fixing a barrel with a solid piston alongside of the common pump, in such a manner, that the lower space of the additional barrel may communicate with the space between the two valves of the pump, and lastly, by connecting the rods so that they may work together. This is shewn in fig. 4, plate XI. and the effect is, that when the pistons are raised, the spaces beneath, A and B, become filled by the pressure of the atmosphere, at the same time that the upper column flows out at E. But again, when the pistons descend, the valve C shuts, and, consequently, the water driven by the piston in B must ascend through A, and continue to produce an equal discharge through E in the down stroke.

XVIII.

Experiments and Observations on certain stony and metalline Substances, which at different Times are said to have fallen on the Earth; also on various Kinds of native Iron. By EDWARD HOWARD, Esq. F. R. S. From the Philosophical Transactions, 1802.

Stony and metalline substances have undoubtedly fallen on the earth.

THE concordance of a variety of facts seems to render it most indisputable, that certain stony and metalline substances have, at different periods fallen on the earth. Whence their origin, or whence they came, is yet, in my judgment, involved in complete obscurity.

The accounts of these peculiar Substances, in the early annals, even of the Royal Society, have unfortunately been blended with relations which we now consider as fabulous; and the more ancient histories of stones fallen from heaven,

* This Gentleman's name was, by mistake, printed Trevithack, in Vol. I. 161.

from Jupiter, or from the clouds, have evidently confounded such substances with what have been termed *Ceraunia*, *Betilia*, *Ombria*, *Brontia*, &c. names altogether unappropriate to substances fallen on our globe. Indeed some mislead, and others are inexpressive. Why these facts have been discredited.

The term *Ceraunia*, by a misnomer, deduced from its supposed origin, seems, as well as *Betilia* *, to have been anciently used to denote many species of stones, which were polished and shaped into various forms, though mostly wedge-like or triangular, sometimes as instruments, sometimes as oracles, and sometimes as deities. The import of the names, *Ombria*, *Brontia*, &c. seems subject to the same uncertainty.

In very early ages, it was believed, that stones did in reality, fall, as it was said, from heaven, or from the gods; these, either from ignorance, or perhaps from superstitious views, were confounded with other stones, which, by their compact aggregation, were better calculated to be shaped into different instruments, and to which it was convenient to attach a species of mysterious veneration. In modern days, Thunderbolt. because explosion and report have generally accompanied the descent of such substances, the name of thunderbolt, or thunderstone, has ignorantly attached itself to them; and, because a variety of substances accidentally present, near buildings and trees struck with lightening, have, with the same ignorance, been collected as thunderbolts, the thunderbolt and the fallen metalline substance have been ranked in the same class of absurdity. Certainly, since the phenomena of lightening and electricity have been so well identified, the idea of a thunderbolt is ridiculous. But the existence of peculiar substances fallen on the earth, I cannot hesitate to assert: and on the concordance of remote and authenticated facts, I shall rest the assertion.

Mr. King, the learned author of *Remarks concerning Stones said to have fallen from the Clouds, in these Days and in ancient Times*, has adduced quotations of the greatest antiquity, descriptive of the descent of fallen stones; and, could it be thought necessary to add antique testimonies to those instanced by so profound an antiquarian, the quotations of Mons. Falconet, in his papers upon *Betilia*, inserted in the *Histoire des Inscriptions et Belles-Lettres*;† the quotations in Zahn's Ancient authorities of stones fallen on the earth. King Falconet, &c.

* Mercati, *Metallototeca Vaticana*. page 241.

† Tom. VI. P. 519. et Tom. XXIII. P. 228.

Specula Physico-mathematica Historiana;* the *Fisica Sotterranea* of Giacinto Gemma; the works of Pliny, and others might be referred to.

Dr. Chaldni on
the Siberian Iron
&c.

Doct^r Chaldni, in his *Observations on the Mass of Iron found in Siberia, and on other Masses of the like Kind*, as well as in his *Observations on Fire-balls and hard Bodies fallen from the Atmosphere*, has collected almost every modern instance of phenomena of this nature.

M. Southey a
stone wt.
10lbs 1796.

Mr. SOUTHEY relates an account, juridically authenticated, of a stone weighing 10 lb. which was heard to fall in Portugal, Feb 19, 1796, and was taken, still warm, from the ground.†

Abbé Bachelay
1768.

The first of these peculiar substances with which chemistry has interfered, was the stone presented by the Abbé Bachelay to the Royal French Academy. It was found on the 13th of September, 1768, yet hot, by persons who saw it fall. It is described as follows:

“ La substance de cette pierre est d’un gris cendré pâle; lorsqu’on en regarde le grain à la loupe, on apperçoit que cette pierre est parsemée d’une infinité de petits points brillans métalliques, d’un jaune pâle; sa surface extérieure, celle qui, suivant M. l’Abbé Bachelay, n’étoit point engagée dans la terre, étoit couverte d’une petite couche très-mince d’une matière noire, boursoufflée dans des endroits, et qui paroissoit avoir été fondue. Cette pierre, frappée dans l’intérieur avec l’acier, ne donnoit aucune étincelle; si on frappoit, au contraire, sur la petite couche extérieure, qui paroissoit avoir été attaquée par le feu, on parvenoit à en tirer quelques-unes.”

The specific gravity of this stone was as 3535 to 1000.

Analyzed.

The academicians analyzed the stone and found it to contain.

Sulphur	-	-	-	-	8½
Iron	-	-	-	-	36
Vitrifiable earth	-	-	-	-	55½

100.

* Fol. 1696, Vol. I. p. 385. where a long enumeration of stones fallen from the sky is given.

† Letters written during a short residence in Spain and Portugal, p. 239.

Of their mode of analysis, I shall have occasion to speak hereafter. They were induced to conclude, that the stone, presented to the Academy by the Abbé Bachelay, did not own its origin to thunder: that it did not fall from heaven; that it was not formed by mineral substances fused by lightning: and that it was nothing but a species of pyrites, without peculiarity, except as to the hepatic smell disengaged from it by marine acid. "Que cette pierre, qui peut-être étoit couverte d'une petite couche de terre ou de gazon, aura été frappée par la foudre, et qu'elle aura été ainsi mise en évidence: la chaleur aura été assez grande pour fondre la superficie de la partie frappée, mais elle n'aura pas été assez long-tems continuée pour pouvoir pénétrer dans l'intérieure c'est ce qui fait que la pierre n'a point été décomposée. La quantité de matières métalliques qu'elle contenoit, en opposant moins de résistance qu'un autre corps au courant de matière électrique, aura peut-être pu contribuer même à déterminer la direction de la foudre."

Conjecture that it might have been pyrites, struck in preference by lightning.

The Memoir is however concluded, by observing it to be sufficiently singular, that M. Morand le Fils had presented a fragment of a stone, from the environs of Coutances, also said to have fallen from heaven, which only differed from that of the Abbé Bachelay, because it did not exhale the hepatic smell with spirit of salt. Yet the academicians did not think any conclusion could be drawn from this resemblance, unless that the lightening had fallen by preference on pyritical matter.*

Another stone from Coutances.

Mons. Barthold, Professeur à l'Ecole centrale du Haut-Rhin, gave I believe the next, and last, † analytical account of what he also denominates *Pierre de Tonnerre*. He describes it thus: "La masse de pierre connue sous le nom de Pierre de Tonnerre d'Ensisheim, pesant environ deux quintaux, a la forme extérieure arrondie, presque ovale, raboteuse, d'un aspect terne et terreux."

Barthold's examination of a stone called thunder stone weighing two quintals.

"Le fond de la pierre est d'une couleur grise bleuâtre, parsemée de cristaux de pyrites, isolés, d'une cristallisation confuse, en quelques endroits écailleuses, ramassés, formant

Description.

* See Journal de Physique, Tom. II. p. 251.

† A very interesting detail of a meteor, and of stones fallen in July 1790, was given by Professeur Baudin, in the *Magazin für das Neueste aus der Physik*, by Professor Voigt.

ON STONY AND METALINE SUBSTANCES, &c.

des nœuds et des petites veines, qui le parcourent en tout sens : la couleur des pyrites est doré ; le poli leur donne un éclat d'acier, et, exposées à l'atmosphère, elles ternissent et brunissent. On distingue de plus, à l'œil nud, de la mine de fer grise, écailleuse, non sulfureuse, attirable à l'aimant, dissoluble dans les acides, peu oxidé, ou s'approchant beaucoup de l'état métallique.

acture.

“ La cassure est irrégulière, grenue, d'un grain un peu ferré : dans l'intérieur on voit de très petites fentes. Elle ne fait pas feu au briquet : sa contexture est si lâche qu'elle se laisse entamer au couteau. En la pilant, elle se réduit assez facilement en une poudre grise bleuâtre d'une odeur terreuse. Quelquefois il se trouve de petits cristaux de mine de fer, qui résistent plus aux coups du pilon.”

The specific gravity of the piece in Professor Barthold's possession, was 3233, distilled water being taken at 1000.

position.

The analysis of Mons. Barthold, of which I shall also have occasion to speak hereafter, gave in the 100.

Sulphur	-	-	-	-	2
Iron	-	-	-	-	20
Magnesia	-	-	-	-	14
Alumina	17
Lime	-	-	-	-	2
Silica	-	.	.	.	42

97

From the external characters, and from his analysis, the Professor considers the stone of Ensisheim to be argillo-feruginous ; and is of opinion that ignorance and superstition have attributed to it a miraculous existence, at variance with the first notions of natural philosophy.*

Sir W. Hamilton's account of stones that fell during a thunder storm.

The account next in succession is already printed in the Transactions of the Royal Society ; but cannot be omitted, as it immediately relates to one of the substances I have examined. I allude to the letter received by Sir William Hamilton, from the Earl of Bristol, dated from Sienna, July 12th, 1794. “ In the midst of a most violent thunder-storm, “ about a dozen stones, of various weights and dimensions, “ fell at the feet of different persons, men, women, and “ children. The stones are of a quality not found in any part

* See Journal de Physique. Ventose, An. 8, p. 169.

“ of the Siennese territory ; they fell about eighteen hours
 “ after the enormous eruption of Mount Vesuvius : which
 “ circumstance leaves a choice of difficulties in the solution
 “ of this extraordinary phenomenon. Either these stones
 “ have been generated in this igneous mass of clouds, which
 “ produced such unusual thunder ; or, which is equally incre-
 “ dible, they were thrown from Vesuvius, at a distance of at
 “ least 250 miles ; judge then of its parabola. The philoso-
 “ phers here incline to the first solution. I wish much, Sir,
 “ to know your sentiments. My first objection was to the
 “ fact itself ; but of this there are so many eye witnesses, it
 “ seems impossible to withstand their evidence.” (Phil. Trans.
 for 1795. p. 103.) Sir William Hamilton, it seems, also
 received a piece of one of the largest stones, which weighed ^{Weight.}
 upwards of five pounds ; and had seen another, which weighed
 about one. He likewise observed, that the outside of every
 stone which had been found, and had been ascertained to have
 fallen from the clouds near Sienna, was evidently freshly
 vitrified, and was black, having every sign of having passed
 through an extreme heat ; the inside was of a light grey
 colour, mixed with black spots and some shining particles,
 which the learned there had decided to be pyrites.

(To be continued.)

SCIENTIFIC NEWS, &c.

Dimensions and Nature of the New Planets Ceres and Pallas.

By Dr. HERSCHEL.

DR. HERSCHEL's observations on the two lately discovered celestial bodies were read before the Royal Society, 6th of May.

Dr. Herschel begins with stating the result of his attempts to measure the diameter of the stars discovered by Piazzi and Olbers. He employed the lucid disc micrometer, which consists of an illuminated circle viewed with one eye, while the other compares with it the magnified image formed by the telescope ; and he concludes, that the apparent diameter of Ceres was 22", and of Pallas 17" or 15", at the distance of nearly 1.634, and 1.187 from the earth respectively, whence the apparent diameters at the distance of the earth from the sun would be .35" and .21" or .16" respectively, and that their

Observation of
the diameters of
Ceres and Pallas.

real

SCIENTIFIC NEWS.

real diameters are about 163 and 95 or 71 English miles.

There is no probability that either of these stars can have a tellite. The colour of Ceres is more ruddy than that of Pallas. They have generally more or less of a haziness, or coma, but sometimes, when the air is clear, this nebulousity scarcely exceeds the scattered light surrounding a very small star. From a view of all these circumstances, Dr. Herschel proceeds to consider the nature of the new stars. He thinks that they differ from the general character of planets, in their diminutive dimensions, in the great inclination of their orbits, in the coma surrounding them, and in the mutual proximity of their orbits; that they differ from comets in the want of eccentricity, and of a considerable nebulousity. Dr. Herschel therefore, wishes to call them asteroids, a term which he defines as a celestial body, which moves round the sun in an orbit either little or considerably eccentric, of which the plane may be inclined to the ecliptic in any angle whatever, the motion being either direct or retrograde, and the body being surrounded or not by a considerable atmosphere or a very small coma. This definition is intended to include such other bodies of the same kind as, Dr. Herschel supposes, will, in all probability, be hereafter discovered. Some additional observations show, that the apparent comas surrounding Ceres and Pallas, scarcely exceed those which are caused by aberration round the images of minute fixed stars.

J. of the Royal Institution.

Extract of a Letter from the Rev. James Wilson, D. D. Minister of Falkirk.

Falkirk, Stirlingshire, June 18th, 1802.

**Durability of
silk buried in
the earth.**

A few weeks ago the sexton of this parish, upon opening a grave in the church yard, found a ribband wrapped about the bone of an arm, which upon being washed was found to be entire, and to have suffered no injury, though it had lain for more than eight years in the earth; and had been in contact with a body which had passed through a state of corruption, and was reduced to its kindred dust. By what means did the silk resist the putrefactive process? it is not a compact substance like hair, horn, or bone, which are frequently found in graves after every other substance is completely changed. As silk is deprived of the gummy matter by the act of cleaning and scouring, and as this seems to be the chief animal sub-

stance

stance which it contains, may not the remaining fibrous part be the better prepared to withstand the power of putrefaction? Accurate experiments tending to illustrate this enquiry might be both amusing and instructive.

ACCOUNT OF BOOKS OF SCIENCE.

Memoirs of the Literary and Philosophical Society of Manchester, Vol. V. Part II. Octavo, 700 Pages, with 9 Plates. Cadell and Davies, London, 1802.

THIS publication of the respectable Society of Manchester is no less interesting than the former volumes of which the scientific world well knows the value; it contains the following memoirs:—1. On tragedy, and the interest in tragical representations: An Essay. By the Rev. George Walker, F. R. S. and Professor of Theology in the new College, Manchester.—2. Experiments and observations to determine whether the quantity of rain and dew is equal to the quantity of water carried off by the rivers and raised by evaporation; with an inquiry into the origin of springs. By Mr. John Dalton.—3. Experiments and observations on the power of fluids to conduct heat; with reference to Count Rumford's seventh essay on the same subject. By Mr. John Dalton.—4. Experiments on the velocity of air issuing out of a vessel in different circumstances; with the description of an instrument to measure the force of the blast in bellows, &c. By Mr. Banks, Lecturer in Natural Philosophy. Communicated by Mr. Dalton.—5. Essay on the beautiful in the human form; and enquiry whether the Grecian statues present the most perfect beauty of form that we at present have any acquaintance with. Communicated to the Society from a Correspondent, through the Rev. George Walker.—6. A defence of learning and the arts, against some charges of Rousseau: In two essays. By the Rev. George Walker, F. R. S.—7. Observations on the nervous systems of different animals; on original defects in the nervous system of the human species, and their influence on sensation and voluntary motion. By John Hill, M. D.—8. Experiments and observations on the heat and cold produced by the mechanical condensation and rarefaction of air.

By

ACCOUNT OF BOOKS.

Mr. John Dalton.—9. Account of some antiques lately found in the river Ribble. By Mr. Thomas Barritt.—10. Experimental essays on the constitution of mixed gases; on the force of steam or vapour from water and other liquids in different temperatures, both in a torricellian vacuum and in air; on fusion; and on the expansion of gases by heat. By Mr. John Dalton.—11. A review of some experiments, which have been supposed to disprove the materiality of heat. By Mr. William Henry.—12. An investigation of the method whereby men judge, by the ear, of the position of sonorous bodies relative to their own persons. By Mr. John Gough. Communicated by Dr. Holme. 13. On the theory of compound sounds. By Mr. John Gough. Communicated by Dr. Holme.—14. Meteorological observations, made at Manchester. By Mr. John Dalton.—Appendix, I. Explanation of a Roman inscription, found in Calfeld-field, Manchester. By Mr. Thomas Barritt. With a note on the same subject, by Dr. Holme.—II. Note to Mr. W. Henry's paper on heat.

THE Rev. Thomas Falconer, A. M. of Bath, proposes to print by subscription, the Geography of Strabo, in seventeen books: translated from the Greek text; illustrated by maps, coins, inscriptions, &c. accompanied with the notes of the older editors, and of the later; those of Thomas Falconer, Esq. of Chester, the Oxford editor, entire; of Siebenkees, and Tzschucke, of Germany; and those of the translator.

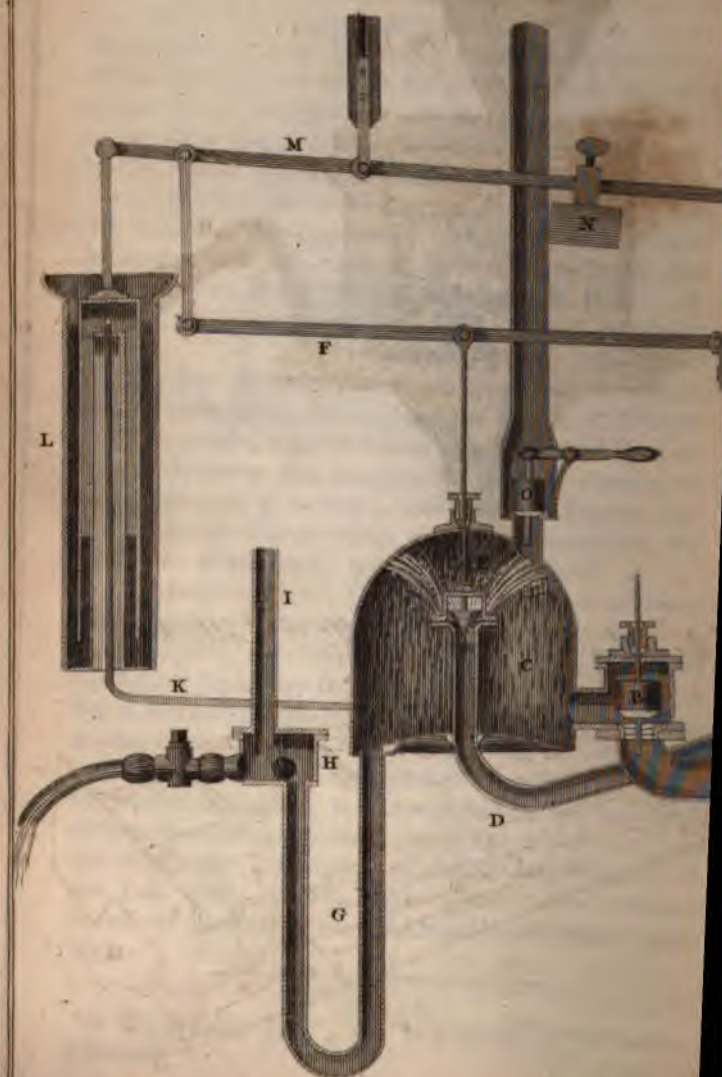
The conditions are—1. The work will be printed in a handsome manner, with foot notes: 2. It will be contained in three volumes quarto, if possible: 3. The price will depend upon the Rate of paper when the work shall be put to press; but it is hoped that four guineas will be the largest estimate: 4. Two guineas to be paid at the time of subscribing, for which a receipt shall be given, and the remainder when half the work is delivered to subscribers: 5. The work will not be sent to press till three hundred copies are engaged, and only five hundred will be printed. Subscriptions received by Messrs. Cruttwell, and Bull, Bath; Cooke, Hanwell, and Parker, Oxford; Cadell and Davies, London; and Manners and Millar, Edinburgh.

ERRATA.

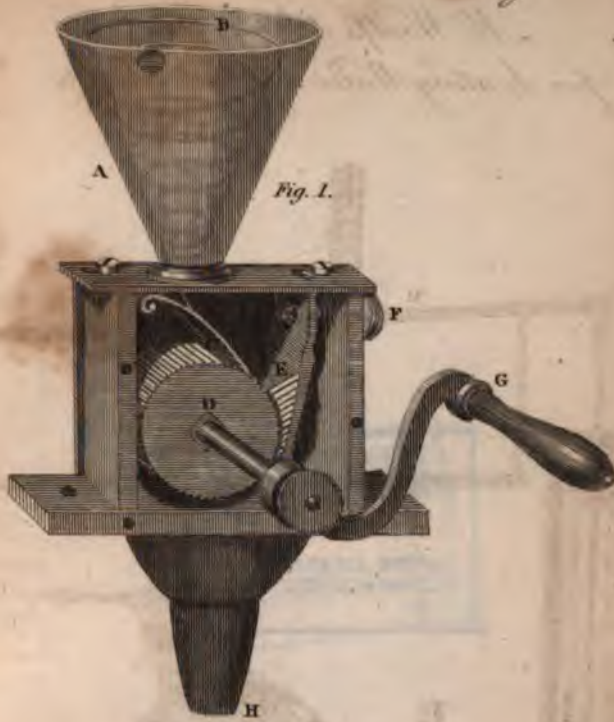
In Mr. Chenevix's paper, p. 114, for *sulphurated* read every where *sulphuretted*.



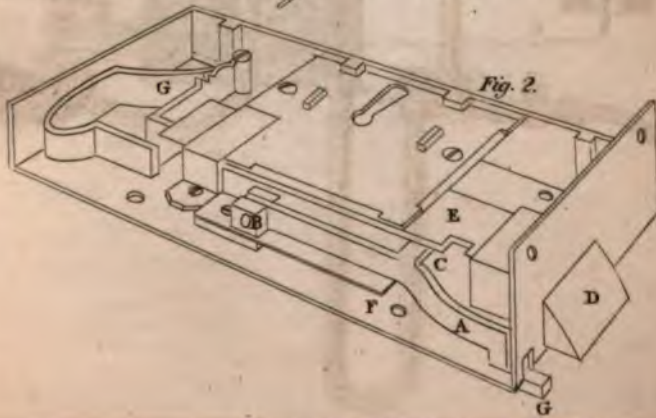
*Mr Woolfs Apparatus
for heating Water by Waste Steam.*



Mill by M.^r Garnet Ferry.



Drawback Lock by M.^r Bullock.

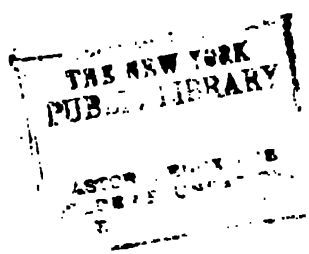


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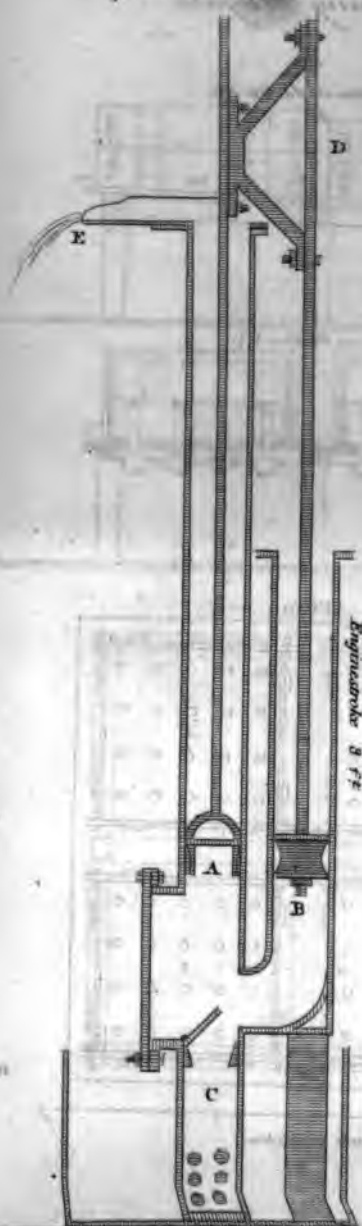
ASTOR, LENOX AND
TILDEN FOUNDATIONS

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*Method of applying a temporary
forcer, by M. Trevithick.*



*Crystals
Fig. 1.*



*described by
C^{te} de Bournon.*

Fig. 2.



Fig. 3.



*Cit. Hapel Lachenaies apparatus
for claying Sugars.*

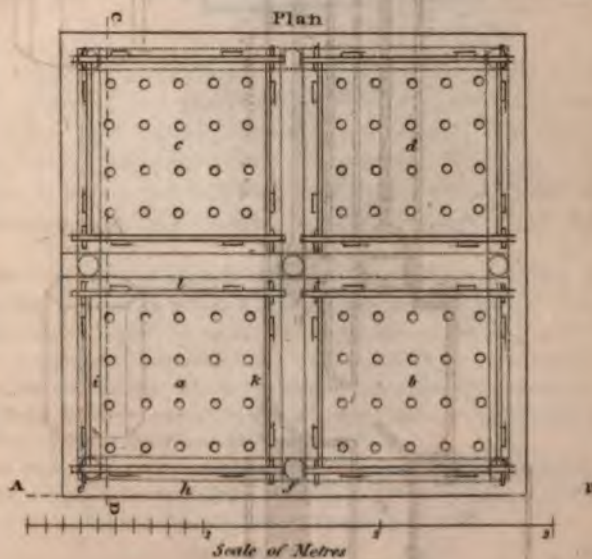
Elevation on the line AB.



Section on the line CD.



Plan



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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

AUGUST, 1802.

ARTICLE I.

On Granite. By Mr. ROBERT JAMESON. Communicated by
the Author.

Sheriff Brae; Leith, July 10, 1802.

THE primitive rocks, of which granite is the oldest, were formed during that period which Werner terms the *chaotic*, when the earth was still covered to a great height with water, and before organization had commenced. Their structure shews that they have been deposited from a state of chemical solution *, and the diminishing level of the newer strata, that

Primitive rocks
formed during
the chaotic state
by deposition
from an aqueous
solvent.

* To the idea of all fossil substances having been in a state of chemical solution in water, it has been objected, that many of them are intirely insoluble in water. To this, without adducing any of the numerous geological proofs, I answer, that we know not the original state of the different earthy and metallic substances; the artificial means we employ to procure them, may, and certainly has altered many of them from their original state.---J.

It may also be remarked, that many insoluble compounds are deposited by the chemical action of bodies which were soluble before they met. Our earths may be such compounds.---N.

VOL. II.—AUGUST, 1802.

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the water has sunk gradually and calmly. They usually occupy the higher parts of the globe, but when covered with newer strata may form the lowest. The rocks which Werner considers as belonging to this great class are, granite, gneiss, mica slate, primitive slate, porphyry, and sienite.

Granite.

As granite is in many respects one of the most important of these formations, I shall here detail several interesting particulars respecting it.

On the name granite.

Pliny, and the older writers, describe this rock under different names; the term granite appears to be of modern date, as Montfaucon is the first writer who uses it. This will not surprise us, when we consider, that it was not until the revival of letters when the remains of antiquity began to be studied, that the different rocks received particular denominations. To Werner we are indebted for the most exact description; before his time it was confounded with sienite and grunstone, two rocks that differ both in their oryctognostical and geognostical characters.

Granite is an aggregate, granular, primitive rock, which is composed of felspar, quartz, and mica.

Aggregate component parts of granite; felspar, quartz, and mica.

Felspar is generally the prevailing, and mica the least considerable ingredient. The felspar has a considerable range of colour; the principal colours are white, grey, red, and sometimes, though rarely, green: it is found in all the intermediate states, from very great to very fine grain. The quartz and mica are generally grey, and the first has sometimes a black colour. Several fossils occur in granite besides those we have just mentioned; these are horn and garnet: such varieties have received particular names, but the geognost views them as accidental, and does not take particular notice of them. The topaz, which is distinguished from all other precious stones (excepting the emerald and garnet) by its occurrence in primitive mountains, is found accompanied with apatite in the tin beds which lie in granite at Ehrenfriedersdorf.

Structure. Porphyritic.

Its *structure* is not subject to much variety. When crystals of felspar are immersed in a basis of fine grained granite, it constitutes what is termed porphyritic granite. Of this there are fine examples near to Carlsbad in Bohemia, and in many places in the north of Scotland. It sometimes occurs in glo-

Globular.

bular

bular distinct concretions *, and these are again composed of concentric lamellar, distinct concretions. This structure of granite is only to be discovered, after the softer granite has weathered out; then these concretions which are vastly harder, and are only separated from each other by the looser granite, make their appearance. Upon the road between Dresden and Bautzen I observed many fine examples of this structure of granite: Mr. Barraud, in his interesting description of the Cape of Good Hope, mentions several globular distinct concretions of immense size. In Scotland the island of Arran affords instances of this kind †.

It is frequently observed distinctly stratified: in other instances owing to the thickness of the strata, this structure is difficultly observable, and has given rise to the opinion that such granite is not stratified. The Riesengebirge, which separate Silesia from Bohemia, are for 150 miles composed of granite, disposed in horizontal strata. Last summer I examined these mountains along with a consummate mineralogist, Dr. Mitchell, and we convinced ourselves of the truth of this observation. I have observed similar stratification in Saxony and Lusatia.

It is an interesting fact in the natural history of granite, that it seldom contains extraneous beds, and Werner remarks, that the frequency of such beds increases with the newness of the formation: thus gneiss contains fewer beds than mica slate, and mica slate fewer than primitive slate.

Limestone, which accompanies all the newer primitive formations, is intirely wanting in granite.

Metals which occur in Granite.

This rock is not so rich in metals and their ores, as the primitive rocks of newer formation. It contains, however, a considerable variety, and some of these have been as yet only discovered in granite. Iron, which is remarkable on account of its occurrence in every period of the earth's formation, is found interspersed in the oldest granite. Red iron ore occurs

* These globular distinct concretions used formerly to be considered as bowlded stones, and afforded an invaluable opportunity for the framing of extravagant hypothesis.

† Mineralogy of the Scottish Isles, vol. i. p. 42.

Molybdena hitherto found only in granite.

Bismuth, cobalt, blende, galena, and particularly tin.

More modern granite, alternating with gneiss.

containing slate.

Veins of granite traversing strata of mica slate, and primitive slate.

Characters of the newer granite; usually low, red, fine grained, &c.

in veins in granite, also the brown iron ore, but this is far seldomer than the red. Molybdena has as yet only occurred in granite, either interperfed, or in veins of the oldeft formation, as at Schlackenwalde, Geya, and Altenberg.

Bismuth, cobalt, blende, galena, and feveral ores of copper have been found in granite. Of all metals, however, tin is the one moft frequently found in granite, and in the great mining field of Cornwall, it is obferved, that copper occurs frequently in primitive flate, but the tin in granite.

The preceding obfervations refer principally to the old granite, which, as far as our experience goes, is the oldeft of all the rock formations. Werner has difcovered other granite formations, which are of a newer date.

1ft. Upon the Schneekoppe, the moft elevated part of the Riefengebirge, which is about 5000 feet above the level of the fea, granite alternates with gneifs, and hence Werner confiders it a diftinct formation*.

2d. At Greifenftein in Upper Saxony, Werner obferved granite which contained pieces of flate lying over ftrata of primitive flate, hence he juftly reckons it to be a diftinct formation, which is newer than either of the preceding.

3d. At Auerberg, near Eibenftock in Saxony, and at Faftenberg near Johangeorgenftald, Werner difcovered veins of granite traversing ftrata of mica flate and primitive flate, and this he is at prefent inclined to confider a new formation. In Scotland granite veins are very common, and feveral circumftances lead me to believe, that thefe and the Auerberg are the fame formation. They are probably both connected with the Greifenftein formation. Werner mentions a few particulars, which he confiders as characteriftical for the newer granite formations.

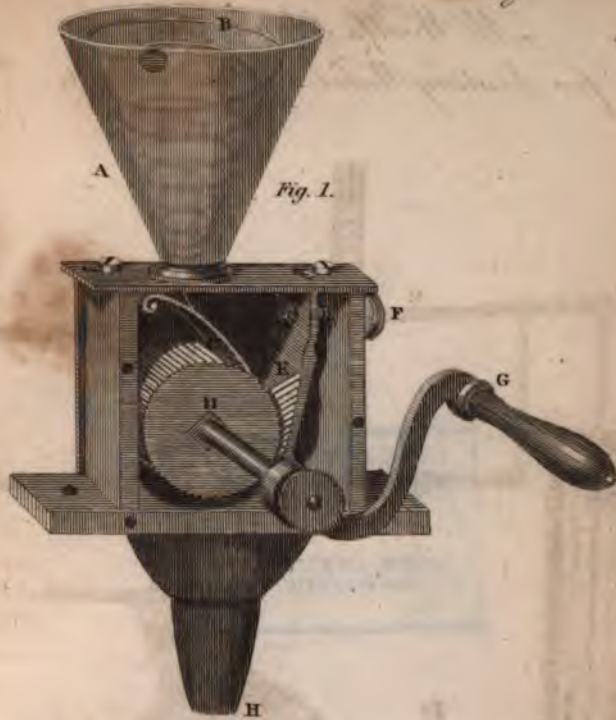
1. Granite, which occurs in low fituations, may be fufpected to belong to the Greifenftein formation.

2. The newer granite has generally a deep red colour, is more frequently fine than coarfe grained, contains garnets, and is not porphyritic.

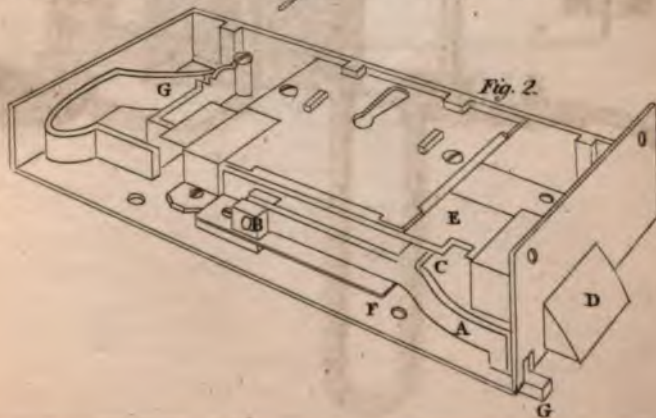
* I was fo fortunate as to have the opportunity of examining this formation in company with Dr. Mitchell. We obferved the granite alternations three times with the gneifs.

The

Mill by Mr. Garnet Ferry.



Drawback Lock by Mr. Bullock.



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ASTOR, LENOX AND
TILDEN FOUNDATIONS

R

L

the line of separation between a mass of granite and the schistus incumbent upon it, all around a tract of country, about eleven miles by seven, extending from the banks of Loch Ken westward; and in all this tract they found, "that wherever the junction of the granite with the schistus was visible, veins of the former, from fifty yards to the tenth of an inch in width, were to be seen running into the latter, and pervading it in all directions, so as to put it beyond all doubt, that the granite of these veins, and consequently of the great body itself, which was observed to form with the veins one uninterrupted mass, must have flowed in a soft and liquid state into its present position*. I have only further to add, that some of these veins are remarkable for containing granite, not sensibly different, in any respect, from the mass from which they proceed." The Criffle in Galloway, which is one of the most considerable portions of what Dr. Hutton considers as the granite of that country, I found to be sienite, consequently it has no relation to the old granite formation. Professor Playfair, who examined the appearances at Loch Ken, believes with Dr. Hutton, that sienite and granite, in a geognostic point of view, are to be considered as the same†. From this I draw the conclusion, that at Loch Ken we have a portion of the same sienite as that which forms the Criffle.

which Mr. Playfair concludes to have flowed in a soft state to their present situation.

The author observed the Criffle to be sienite,

and infers that the other granites are a portion of the same.

The same inference generally stated,

It appears then evident, that wherever granite, in the form of veins is to be observed issuing from granite into the contiguous strata of gneiss, mica slate, &c. it must belong to a newer formation, and probably to that of Greifenstein. Many of the instances where such appearances have been observed, certainly belong to the sienite formation.

It is therefore demonstrated, that Granite is the oldest Rock with which we are acquainted,

and that granite is the oldest of rocks.

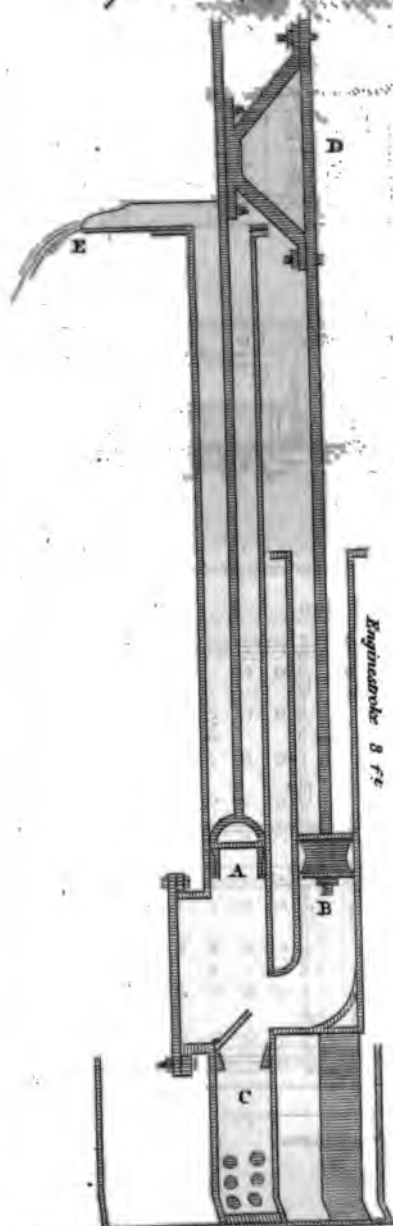
Before I conclude these remarks, I shall notice two objections which have been urged against the possibility of granite veins having been filled from above. Mr. Playfair observes at page 313, "that a strong objection to the supposed origin of granitic veins from infiltration, and indeed to their formation in any way but by igneous fusion, arises from the number of

To Professor Playfair's remark that fragments of schistus could be insulated in granite veins only by igneous fusion;

* Transactions of the Royal Society of Edinburgh, vol. iii. p. 8.

† Illustration of the Huttonian Theory, p. 312.

*Method of applying a temporary
forcer, by M. Trevithick.*



*Crystals
Fig. 1.*



*described by
C. de Bournon.*

Fig. 2.

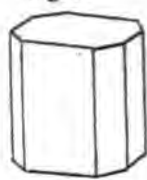
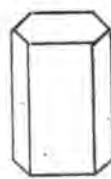


Fig. 3.



Engine 8 ft

*Cit. Hapel Lachonaries apparatus
for claying Sugars.*

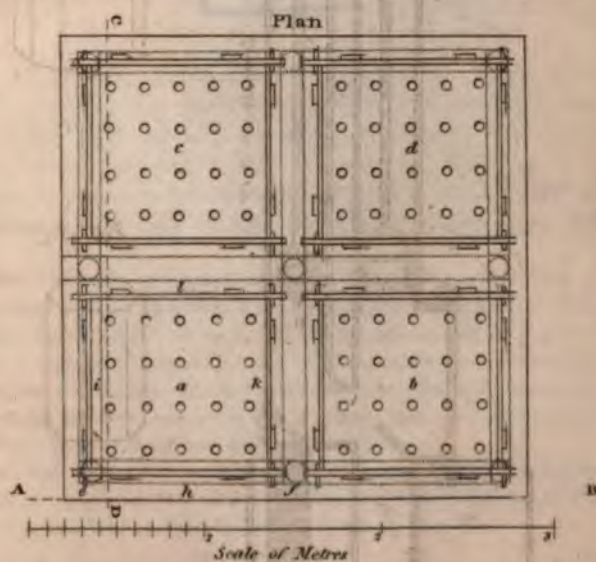
Elevation on the line AB.



Section on the line CD.



Plan



which they imbibed and became bright.

These left in vitriolic acid a black matter as steel does.

Its habitudes were those of finery cinder.

The black matter from pure steel was affected in the same manner.

It was therefore finery cinder, and not carbon.

Bright needles afforded much less black matter than watch springs, of which the surface was blued or oxidized.

measures of it. In consequence of this, from being of a dark colour, they became exceedingly bright; and I concluded that they were now become *steel*, though I was not able to ascertain it by a direct experiment. But after dissolving those bright filings in diluted vitriolic acid, a quantity of black matter, as after the solution of steel, remained unaffected by it. This being heated in inflammable air imbibed a considerable quantity of it, and then, by means of diluted vitriolic acid, gave inflammable air very copiously. This black matter had evidently the properties of finery cinder.

I then dissolved 200 grains of *broken watch springs*, which are undoubtedly pure steel, and collected from the solution three grains of black matter. Heating this in inflammable air, a great proportion of it was imbibed; and then, by means of the diluted acid, it gave out inflammable air as copiously as iron or steel filings would have done. This black matter, therefore, from the solution of steel was finery cinder, and not carbon, or plumbago. And as iron acquires weight by becoming finery cinder, and this addition of weight, I think I have proved to be from *water*, it can hardly be doubted, but that the addition of weight to iron, in being converted into steel, is from the same cause. Indeed, I believe it to be impossible to expose iron to a red heat in circumstances in which there is any possible access of water, or of air, which always contains water, without a partial calcination of it; that is without its becoming superficially at least finery cinder.

This was evidently the difference between the result of the solution of the *watch springs*, and that of an equal weight, viz. 200 grains of *broken polished needles*, which had not undergone any calcination. For the black matter that remained from the solution of them would not have weighed a quarter of a grain. Giving colour to steel, which is done to watch springs, is always a partial calcination of the metal; and this appears from the preceding experiments to be the conversion of a part of it into finery cinder, which is the reverse of plumbago; being, according to the new theory, an *oxide* in the highest degree; whereas if plumbago contain any oxygen, it is in the lowest degree.

III.

*An Account of the Art of making Glue. In a Letter from
Mr. JOHN CLENNEL.*

DEAR SIR,

Newcastle, June 21, 1802.

THE following attempt to develope the "art and mystery" of glue-making is at your service, if you think it worthy a place in your very valuable miscellany. The improvement of any manufacture depends upon its easy access to men of science, and a prudential theorist can never be better employed than in attempting to reduce to regularity or to system the manufactures that may fall under his attention. In conformity to the first principle, I made some notes whilst visiting a glue manufactory a few years ago in Southwark, and those, interwoven with the remarks on that subject of some chemists of the first respectability, I take the liberty of sending you: at the same time I must beg of you, or your correspondents, that where it may be corrected in any manner, it may be done, and I shall feel myself obliged by the attention.

Glue is an inspissated jelly, made of the parings of hides or horns of any kind, the pelts obtained from furriers, and the hoofs and ears of horses, oxen, calves, sheep, &c. quantities of all which are imported in addition to the home supply, by many of the great manufacturers of this article: these are first digested in lime water, to cleanse them as far as it can from the grease or dirt they may have contracted; they are then steeped in clean water, taking care to stir them well from time to time; afterwards they are laid in a heap, and the superabundant water pressed out; then they are boiled in a large brass caldron with clean water, skimming off the dirt as it rises, and further cleansed by putting in, after the whole is dissolved, a little melted alum or lime finely powdered, which, by their deterfivè properties, still further purge it: the skimming is continued for some time, when the mass is strained through baskets, and suffered to settle, that the remaining impurities, if any, may subside; it is then poured gently into the kettle again, and further evaporated by boiling a second time, and skimming, until it becomes a clear but darkish brown colour: when it is

Great advantages
of the visitation
and report of
manufacturers.

Glue is made
from refuse of
hides, &c.

Cleaned from
grease by lime-
water; then
steeped in clean
water; drained;
boiled in water;
clarified by alum
or lime; strain-
ed; cleared by
subsidence;
again boiled to
the requisite
density; poured
into moulds;
cut into por-
tions; and dried
on a net.

thought

thought to be strong enough (which is known either by the length of time a certain quantity of water and materials have boiled, or by its appearance during ebullition), it is poured into frames or moulds of about six feet long, one broad, and two deep, where it hardens gradually as the heat decreases: out of these troughs or receivers it is cut when cold by a spade, into square pieces or cakes, and each of these placed within a sort of wooden box, open in three divisions to the back; in this the glue, as yet soft, is taken to a table by women, where they divide it into three pieces * with an instrument not unlike a bow, having a brass wire for its string; with this they stand behind the box and cut by its openings, from front to back; the pieces thus cut are taken out into the open air, and dried on a kind of coarse net work, fastened in moveable sheds of about four feet square, which are placed in rows in the glue-maker's field (every one of which contains four or five rows of net work); when perfectly dry and hard, it is fit for sale.

character of
good glue.

judgement of
the cakes.

That is thought the best glue which swells considerably without melting, by three or four days immersion in cold water, and recovers its former dimensions and properties by drying. Glue that has got frost, or that looks thick and black, may be melted over again and refined, with a sufficient quantity added of fresh to overcome any injury it may have sustained; but it is generally put into the kettle after what is in it has been purged in the second boiling. To know good from bad glue, it is necessary for the purchaser to hold it between his eye and the light, and if it appears of a strong dark brown colour, and free from cloudy or black spots, the article is good.

I am, Sir,

With great respect,

Your's, &c.

JOHN CLENNELL.

* When the women, by mistake, cut only two, that which is double the size is called a Bishop, and thrown into the kettle again.

IV.

*On the Preparation of Indelible Ink. In a Letter from
Mr. THOMAS SHELDRAKE.*

To Mr. NICHOLSON.

S I R,

AS your correspondent, Mr. Close, has alluded to my memoir on the nature and preparation of drying oils, it may not be unpleasant to receive such information as I am able to give respecting the object of his pursuit.

By experiments repeated and varied every way that my imagination can suggest, I am convinced that amber is not soluble in alcohol or any essential oil: it is soluble in expressed oils, by the process described in Lewis's philosophical commerce of the arts, but that solution does not dry well, and therefore will not answer Mr. Close's purpose; but when dissolved by the * well-known process for making amber-varnish, it is likely to answer extremely well.

There is another substance which seems likely to answer his purpose very well. † Asphaltum is a bituminous substance, perfectly black when viewed in a mass, but a dark transparent brown when dissolved: it is soluble in spirit of turpentine at a

Amber is not soluble in alcohol or essential oil.

The solution in expressed oil is not good for ink; but the solution for varnish is good.

Asphaltum promises to be a good material.

* The following is the most convenient method: Put small pieces of amber into an iron ladle, set it on a fire till they are melted, then add so much of the best drying oil as will make it liquid, stir them well together, and, when cold, add so much spirit of turpentine as will make it thin enough to flow from the brush. The object in making varnish is to discolour the amber as little as possible; therefore it is but little roasted, and the lightest coloured drying oil is used; but if this solution of amber was used for making ink, the darkness of the colour would be an advantage, therefore the amber should be thoroughly melted, and the darkest drying oils used in preference to the others.

Process for the oily solution of amber.

† Within these few years good asphaltum may be procured in many shops in London: before that period it was unknown; the best of what was sold, and is still sold in some places, was the caput mortuum of amber; other compositions of pitch and various resins were likewise sold for asphaltum. If it were used for Mr. Close's ink, care should be taken to select the best.

Good asphaltum is now easily procured.

low

low heat, and when dissolved runs freely from the pen: I have known some artists draw with this in preference to ink, because its colour harmonizes better with other materials also used in drawing, and because it is indelible, as it strikes immediately into the paper, and if it is not thick, will strike through it: by this means every stroke made with it is visible through every colour that is washed over it.

Asphaltum in spirit of turpentine, rendered consistent with solution of amber, and coloured with lamp black, would give an indelible ink.

The drying oil of the varnish would increase the difficulty of obliteration.

It seems then that if a solution of asphaltum was made in spirit of turpentine, and so much of the solution of amber was added as would give it due consistence, and the finest lamp black to give it colour, a perfect ink would be formed, and possessing those properties Mr. Close seems to desire; for, supposing the other materials could be removed, so much of the colour as depends on the asphaltum would be indelible, except by such means as would destroy the paper or parchment.

Even the small quantity of drying oil introduced into the varnish, would be useful in this respect; for it is well known, that if oil is dropped upon white paper, though the mark is scarcely visible at first, in a year or two it will become a dark yellowish brown: it seems as though the oil changes the paper so much that its colour can never be recovered, at least those who undertake to restore the white colour of old prints, always make an exception to spots of oil.

If this hint should be thought deserving any notice, you will have the goodness to make what use you please of it.

I am, Sir,

Your's, &c.

T. SHELDRAKE.

No. 50, Strand, July 6, 1802.

Copal would probably be discharged from paper by camphorated spirit.

P. S. I believe Mr. Close will find himself mistaken as to the insolubility of copal when used in his ink. So powerful is the influence of camphor upon it, that if copal be reduced to powder, and a little camphor is rubbed into it, it immediately begins to soften, and the whole soon becomes a coherent mass; and though copal is not soluble in alcohol alone, if camphor is added, it dissolves as easily in the compound as the softest resin would. It is therefore extremely probable, that, if a paper written with his ink was washed with camphorated spirits, the writing would be removed with very little difficulty.

V. Observations

V.

Observations on the Causes why a large Quantity of common Salt prevents Putrefaction, and a small Quantity hastens it. By D. H.

To Mr. NICHOLSON.

S I R,

THERE are few phenomena of nature more interesting, and at the same time more involved in obscurity, than the two opposite actions of muriate of soda, which is known to have very considerable effect, both in accelerating and in retarding putrefaction. The *antiseptic* property of this salt has been known from the earliest ages. It was discovered, however, by Pringle, Macbride, and Gardane, that putrefaction may be hastened by sprinkling the animal substances with water holding a small quantity of muriate of soda in solution. This discovery excited much surprise; and the celebrated chemists who observed it seem to have been fully aware of the difficulties attending its explanation, as they have offered no theory to account for it. It would doubtless appear a presumptuous undertaking, to attempt the solution of a question which has baffled the ingenuity of so many philosophers, did not the subsequent discoveries in chemistry and physiology enable us to speculate on the subject with some degree of probability.

It seems necessary for the decomposition of an animal substance, 1st, that it be in contact with atmospheric air; 2^{dly}, that it be exposed to a moderate degree of heat; and, 3^{dly}, that it be impregnated with humidity. It must necessarily follow, that whatever removes these conditions will check the progress of putrefaction. Of this we have many instances, as in the effects of cold; in covering the substances with sugar, resins, &c. and in preserving them in spirit of wine. I conceive with Gren, that muriate of soda acts only in this way, by abstracting the moisture, and removing the substance from the contact of oxygen; and not by a peculiar innate, and as it were hidden (*vis occulta*) antiseptic power*.

Remarkable opposition of effects of common salt in preventing or accelerating putrefaction; as its quantity is more or less.

Putrefaction requires air, warmth, and moisture;

and is checked by cold and covering from the air.

Crude common salt supposed to preserve by drying and covering the body it is applied to.

* Gren's Chemistry, Chap. VIII.

With

Putrefaction is hastened by destroying irritability.

Common salt inferred to produce that effect.

Much salt retards putrefaction by drying and covering, more than it accelerates by the last mentioned quality.

Little salt accelerates it for the contrary reason.

With regard to the *septic* property of muriate of soda, it must be referred to another cause. The destruction of muscular irritability appears to be a chief cause of accelerating putrefaction. This was ascertained by Mr. John Hunter, who found, that when death is occasioned by an electric shock, by violent exercise, by a blow on the stomach, or by any thing that destroys the irritability of the muscular fibre, putrefaction quickly ensues. Fontana found that the same effects were produced by the poison of vipers. It has been also found by experiment, that the compounds of potash and soda destroy muscular irritability. Now, is it not a fair inference from these premises, that a small quantity of muriate of soda should possess a septic quality? Upon these grounds, it will not be difficult to reconcile the two opposite actions of muriate of soda. When a large quantity of this salt is applied to an animal substance, it acts merely by removing the indispensable conditions of putrefaction, air and moisture. The particles in contact with the substance may indeed act in destroying the irritability of the muscular fibre; but this being only a secondary cause of putrefaction, cannot operate unless in conjunction with air and moisture. On the other hand, when a small quantity of this salt in solution is applied, it is insufficient either to exclude the air, or to abstract the moisture; its peculiar property, therefore, acts *in conjunction* with the other causes, and these causes united accelerate the putrefactive process much more than any of them separately.

Such is the explanation of these phenomena which occurred to me. Although it may be imperfect in many points, yet it appears to involve no hypothesis, but to be a strict deduction from facts. Should this attempt have the good fortune to meet with your approbation, its insertion in the Philosophical Journal may at least have the effect of drawing the attention of some more eminent chemist to this too much neglected subject.

I am, Sir,

Your's respectfully,

Edinburgh, July 12, 1802.

D. H.

VI.

Account of the Methods by which Soda is at present prepared for the English Market; with other Observations. By Mr. FRED. ACCUM. From the Author.

SINCE the late new duty on salt, the manufacturers of soda avail themselves of the method of decomposing the sulphate of soda *, by means of what is called American potash. Though this kind of potash contains less pure alkali than the Russian or German potash, it is imported in a state of perfect dryness, whereas the other is always met with in a moist state; so that the absence of water appears to compensate for the deficiency of the alkali. The American potash sells now from 48s. to 54s. per cwt. it has been considerably higher, until lately it fluctuated between 54s. and 56s.

Soda obtained by decomposing the sulphate of the bleachers with American potash.

I have been employed in a soda manufactory in which the following method answered exceedingly well. Five hundred pounds of sulphate of soda were introduced into an iron boiler containing a sufficient quantity of Thames water. 560 lb. of American potash were likewise dissolved in as little water as possible in an iron boiler fixed near the former. The potash was always previously tried, and if indifferent the quantity taken was 10 lb. more.

Particular description of the manufacturing process.

The solution, as near as I recollect, was made with about thirty pails of water to the alkali here mentioned. Both solutions were then made to boil, and as soon as the ebullition took place the solution of potash was ladled into the boiler containing the sulphate of soda. The mixture was agitated during the transfusion, and the fire raised as expeditiously as possible. As soon as the fluid boiled it was ladled into a wooden gutter, which conveyed it into a cistern of wood lined with sheet lead nearly half an inch thick, which was fixed in a cool place. Sticks of wood were then placed across the cistern, from which slips of sheet lead two or three inches wide were hung into the fluid at four inches distant from each

A boiling solution of potash was added to another of sulphate of soda,

then boiled,

* Sulphate of soda is sold cheap by the bleachers, who have it as the residue of decomposing common salt by sulphuric acid with manganese. In February, 1801, it was worth from 11s. to 14s. per cwt. but I have not the present price.---A.

VOL. II.—AUGUST, 1802.

R

other.

and drawn off to crystallize in a leaden cistern.

at a temperature under 55° . The soda is then washed with cold water;

then dissolved and evaporated at a low heat.

Pellicles of sulphate of potash are formed and fall down;

which ceasing, the fluid is drawn off to crystallize.

100 parts of sulphate of soda afford 138 of carbonate.

The two crystallizations are found to be necessary.

other. When all was cool, which in the winter was generally the case in three days, a plug in the bottom of the cistern was drawn in order to let off the fluid, and the crystallized salt was taken from the slips of lead. The bottom exhibited a rock of salt, which was detached by chissel and mallet. On this account it is that the lead which lines the cistern must be thick, in order to guard against accidents. For if the metal be perforated, the saline solution creeps between it and the wood, and in a very short time detaches the lining, and it is besides extremely difficult to find out the place where the defect really is. The temperature where the soda is left to crystallize ought not to exceed 55° Fah. In this stage of the process the whole of the salt is washed in the same cistern with cold water, to clear it of impurities; after which it is transferred again into the boiler, dissolved in clear water, and evaporated by heat. As soon as a strong pellicle is formed, it is suffered to cool so far that the hand may be dipped in the fluid without injury, and the heat is kept at that temperature as long as effectual pellicles continue to be formed over the whole surface of the boiler, and then fall to the bottom. When no more pellicles are formed, or at least only by blowing with the mouth upon the surface, the fire is withdrawn, and the fluid is ladled out into the cistern to crystallize. The sulphate of potash, &c. which had been deposited, is then taken out of the boiler and put aside. If the fluid be suffered to cool pretty low before it is suffered to run into the cistern, very little sulphate of potash is found in the soda; but in general the rocky masses of soda met with in the market contain a considerable quantity. By this process from 136 to 139 lb. of soda may be obtained from 100 lb. of sulphate of soda, if the soda be crystallized in large crystals; if small crystallized it yields less; it sells now at 52s. or 54s. per cwt. and is retailed at 8d. per lb.

We might be inclined to suppose that the first operation was unnecessary, and that the soda might be separated at once from the sulphate of potash at the instant of its formation; but practice will convince the operator otherwise. A considerable loss is manifested if the process be not conducted in this manner; though the discovery of the cause may perhaps be not so easily accomplished as the proof of the fact.

Other

Other manufacturers grind together 500 cwt. of Glauber's salt of the bleachers, and 100 cwt. of charcoal; they expose this mixture in a reverberatory furnace resembling a baking oven, till the matter when stirred with a rake becomes pasty. It is then withdrawn and transferred into large casks, each provided with a double bottom. Water is then suffered to stand one inch high over it for 24 hours; the cock is then opened, the solution runs through the perforated bottom, over which a stratum of straw had been previously placed; and is thence conducted into the boiler for evaporation and crystallization *.

Another process: The acid of sulphate of soda is decomposed and expelled by heat with charcoal,

and the soda extracted by water, &c.

It is a curious fact, that iron plates are absolutely necessary to constitute the surface on which these articles are exposed to heat: fire bricks do not answer. It seems as if the iron assisted the union; though neither iron filings mixed with the articles nor pyrites are found of advantage.

The heat must be applied on an iron, and not a brick hearth.

This method of making soda is extremely uncertain. If the heat be not raised gradually, or if the mixture be not fused enough, or a little too much, it does not succeed. The worst event is, that when the mixture has been made too hot, sulphuric acid is produced, and sulphate of potash is formed.

Uncertainty of this method.

The quantity of soda which may be obtained by this process, is said to be equal to that obtained by any other method.

It is said to be profitable if well managed.

I have been lately informed, that in Germany soda is made by decomposing the sulphate of soda by means of acetite of lime; the acetic acid is obtained for that purpose from wood, and the charcoal is found to pay the costs.

Third method. Sulphate of soda decomposed by acetite of lime, of which the acid is had by distilling wood. The method recommended by acetite of lead, or else oxides of that metal is productive of loss in this country. The muriate of lead thus obtained is not a pigment.

The method recommended by several chemists, of obtaining soda by decomposing Glauber's salt by acetite, or the oxides of lead, does not answer in this country. The mass is by far too bulky; and requires too much time, attendance, and fuel to reduce it to a narrow compass. I have been informed by men well skilled in this department, that it is nearly impracticable in the large way. The muriate of lead which is produced cannot be used as a white pigment, as the inventors pretend.

In Prussia they mix muriate of soda and quicklime together; then slake the lime, and form the whole into a thick pulp,

Prussian method by quicklime; then slaking and long exposure to the air. Product sulphate of lime and carbonate of soda.

* This was the process at the salt works.---N.

which is extended about two inches thick over a large surface, and left in that situation for three months. Carbonate of soda is then formed, which is washed out and crystallized in the usual manner. The soda obtained by this process always has a yellowish cast.

VII.

Comparison of the French definitive Metre with an English Standard, brought from London by M. A. PICTET, one of the Editors of the Bibliothèque Britannique.*

Short history of
admeasurements
of the earth.

particularly that
lately made in
France.

and the standard
measure thence
deduced.

THE measurement of the earth, and the investigation of its figure, were the subjects, at various times in the course of the eighteenth century, of the labours of a number of philosophers of the first eminence in different countries. Some Swedish astronomers are now employed in a second measurement of the same degree which was measured sixty years ago by the French Academicians in Lapland, under the polar circle. In France, when the idea of seeking in the dimensions of the globe itself the unit to which all measures and weights might be referred, had once been conceived and adopted, it was necessary to make an effort proportional to the importance of an undertaking which was thus become national. In the midst of a long and sanguinary war, together with difficulties of every other kind, a chain of triangles has been formed between Dunkirk and Barcelona, comprehending the tenth part of the arc of the meridian which extends from the Equator to the pole, and which is equal to one fourth of the circumference of the globe; and the ten millionth part of this arc, thus determined, has been adopted for the unit of the metrical system: it has been fixed by the construction of standards made of substances proper to resist the attacks of time; and by a careful examination of the precise relation of the length of the metre to that of the pendulum vibrating seconds, on the level of the sea, in a given latitude, the determination of this unit has been rendered independent of any accident that might destroy or

* From No. 148 of the Bibliothèque Britannique. I avail myself of the free translation given in the Journal of the Royal Institution; but have very carefully read the proof with the original.--N.

impair the standards representing it; while in the formation of these standards all the precautions have been employed that could be suggested by the present improved state of natural philosophy, and of the arts.

In England, on the other hand, operations have been carried on for these five and twenty years, which are to be the foundations of an exact map of Great Britain. These labours, begun by the late General Roy, have been conducted with much sagacity and precision; and the results are likely to procure very interesting information respecting the figure of the earth. Sir George Shuckburgh, an eminent member of the Royal Society of London, has successfully employed himself in private, in researches intended to fix the precise length of the standards, which have served as bases for the measurements made in Great Britain.

Operations of the same kind in England.

It was therefore to be regretted, that operations so similar, conducted in two neighbouring countries, and capable of acquiring a new interest by comparison, should remain unconnected, for want of an actual standard of the measures of the one country, which might be transported into the other, after the definitive determination of the French measure. This regret we had deeply felt at various times when these objects were laid before our readers; and we may say with truth, that if the hope of procuring this medium of comparison was not the only motive of the journey to England that one of us has made, it at least greatly contributed to induce him to undertake it.

Great advantage of an accurate comparison of the French and English standards.

Our colleague took some steps in his passage through Paris, to obtain an authentic metre, in order to be submitted to the examination of the Royal Society, to which he has the honour of belonging, but he did not remain long enough in Paris to be able to succeed in this attempt. He took advantage of his longer stay in England, in procuring from the hands of Mr. Troughton, an artist celebrated for his accuracy in the construction and division of geometrical and astronomical instruments, a standard rigorously conformable to that which he had made for Sir George Shuckburgh, and with which this philosopher had compared the principal English standards. Our colleague procured also from the same artist the comparative apparatus of Sir George Shuckburgh, composed of two excellent microscopes, the one bearing a micrometer which divides

Means used by M. Picquet to obtain a true British standard.

Apparatus by Troughton for comparing linear measures.

Commission of
the French National Institute
for comparing.

vides the English inch into ten thousand equal parts. Upon his return to Paris he made haste to exhibit these instruments to the Minister of the Interior, and to the National Institute. This learned body nominated three of its members, in order to proceed to the regular comparison of the definitive metre with the English standard. The undertaking, by no means so easy as it at first appeared, occupied the committee in five different meetings, of nearly four hours each; and it was performed with all the care and precaution that the nature of the subject required. Mr. Prony, who, as the translator of General Roy's memoir on the first trigonometrical operations in England, was particularly interested in these researches, acted as secretary to the Committee, and it was at his house, and with the assistance of a comparative apparatus belonging to him, that the principal experiments were made. He has been so obliging as to furnish us with an authentic copy of the report made to the Institute, which was deemed of sufficient consequence to be read at the public sitting of the last quarter. He adds, that "This report will soon be followed by a memoir, in which he will enter into more circumstantial details of all the observations that he has made; and in which he will give a description and a figure of his comparative instrument." We shall bear in mind this promise, and in the mean time we shall give our readers a copy of the report; informing them that we have bestowed on the correction of the proofs of this important paper all the attention necessary to enable us to affirm that no typographical error has been committed in the numbers.

*National Institute of Sciences and Arts, 6 Nivose, Year 10
(27th December, 1801.)*

Report of the
Institute.

A member read, in the name of a committee, the following report on the comparison of the standard metre of the Institute with the English foot.

M. Picet, Professor of Natural Philosophy at Geneva, submitted to the inspection of the class in the month of Vendémiaire, an interesting collection of objects relative to the sciences and arts, which he collected in his journey to England.

English standard
of 49 inches,

Among them was a standard of the English linear measure, engraved on a scale of brass, of 49 inches in length, divided by very fine and clear lines into tenths of an inch.

It

It was made for M. Piçet by Troughton, an artist in London, who has deservedly the reputation of dividing instruments with singular accuracy; it was compared with another standard made by the same person for Sir George Shuckburgh, and it was found that the difference between the two was not greater than the difference between the divisions of each; that is, it was a quantity absolutely insensible. This standard may therefore be considered as identical with the standard described by Sir George Shuckburgh in the Philosophical Transactions for 1798.

M. Piçet also exhibited to the Institute a comparer, or an instrument for ascertaining minute differences between measures, constructed also by Mr. Troughton. It consists of two microscopes with cross wires, placed in a vertical situation, the surface of the scale being horizontal, and fixed at proper distances upon a metallic rod. One of them remains stationary at one end of the scale, the other is occasionally fixed near to the other end; and its cross wires are moveable by means of a screw, describing in its revolution $\frac{1}{1000}$ of an inch, and furnished with a circular index, dividing each turn into 100 parts; so that having two lengths which differ only one tenth of an inch from each other, we may determine their difference in ten thousandths of an inch. The wires are placed obliquely with respect to the scale, so that the line of division must bisect the acute angle that they form, in order to coincide with their intersection. General Roy has described, in the 75th volume of the Philosophical Transactions, a similar instrument made by Ramsden, for measuring the expansion of metals.

M. Piçet offered to the class the use of the standard, with the micrometer described, for the determination of the comparative length of the metre, and the English foot: the offer was accepted with gratitude, and M M. Legendre, Méchain, and Prony, were appointed to co-operate with M. Piçet in the comparison of the standard metre of platina and the English foot.

The first meeting was on the 28th Vendémiaire (21st of October,) at the house of Mr. Lenoir.

At first a difficulty occurred from the different manner in which the measures were defined: the English scale was graduated by lines; the French standards were simply formed to the length of a metre: hence the length of the metre could not easily be compared with the English scale graduated by lines, and the French, which is of the precise

length without graduation.

easily be taken by the microscopes; nor could the English scale be measured by the method employed for making new standard metres, which consists in fixing one end against a firm support, and bringing the other into contact with the face of a cock or slider, adjusted so as barely to admit the original standard between it and the fixed surface.

Method by an intermediate piece.

Mr. Lenoir attempted to overcome this difficulty by reducing to a thin edge the terminations of a piece of brass of the length of a metre; so that it was compared with the standard metre in the usual manner, and its extremities, when placed on the English scale, constituted two lines parallel to those which were really engraved on the scale, and capable of being viewed by the microscopes.

Experimental comparison by this method.

The standard metre of platina, and another standard of iron, belonging also to the Institute, were thus compared with the English foot; each of these two measures being equal, at the temperature of melting ice, to the ten millionth part of the quadrant of the meridian. At the temperature of 15.3° of the decimal thermometer, or 59.5° of Fahrenheit, the metre of platina was equal to 39.3775 English inches; and that of iron to 39.3788, measured on M. Picet's scale.

Result,

in some respects uncertain.

These first experiments, showed, however, that the method employed was liable to some uncertainty, arising from the difficulty of placing the cross wires precisely at the extremity of the thin edge of the plate of brass employed in the comparison; a reflection or irradiation of light, which took place at that extremity, prevented its being distinctly observed if the optical axis of the microscope was precisely a tangent to the surface exactly at the termination.

Accurate method of comparison by contacts; a ruler carrying a line was caused to move through the precise length of the metre, under the microscopes.

In order to remove this inconvenience, another arrangement was proposed by one of the Committee. (It was Mr. Prony that suggested this ingenious method, and M. Paul of Geneva, who happened to be present, that executed it. B.B.) A line was traced on a small metallic ruler, perpendicular to its length; the end of the ruler was fixed against a solid obstacle, and the cross wires made to coincide with the line: the standard metre was then interposed between the same obstacle and the end of the piece, and the line traced on it, which had now obviously advanced the length of the metre, was subjected to the other microscope. To the microscopes

thus

thus fixed, the graduated scale was transferred; one of the divisions was placed exactly under one of the microscopes, and the micrometer screw was turned in order to measure the fraction, expressing the distance of the other microscope from another division.

The comparison was repeated in the same manner the 4th Brumaire (26th October last) at the house of one of the Committee, and after several experiments, agreeing very satisfactorily with each other, it was found that at the temperature 12.75° centigrade, or 55° of Fahrenheit, the standard of platina was 39.3781, and that of iron 39.3795 English inches. Precise result at 55° of Fahrenheit.

The two metres being constructed to be equal at the temperature of melting ice, these operations may be verified by reducing their results to that temperature. For this determination we are provided with the accurate experiments made by Borda, and the committee of weights and measures, on the dilatation of platina, brass, and iron; from which it appears, that for every degree of the decimal thermometer, platina expands .00000856; iron .00001156; and brass .00001783; (for Fahrenheit's scale these quantities become 476, and 642, and 990 parts in an hundred millions.) From these data we find that, at the freezing point, the standard metre of platina was equal to 39.38280, and that of iron to 39.38265 English inches of M. Picot's scale. The difference is less than the 500th of a line, or the 200000th of the whole metre, and is therefore wholly inconsiderable. Reduction to the temperature of melting ice.

The result of the whole comparison is therefore this. Supposing all the measures at the temperature of melting ice, each of the standard metres is equal to the 10000000th part of the quadrant of the meridian, and to 39.38272 English inches of M. Picot's scale. Standard metre at 32° is = 39.38272 English inches.

At the class of mathematical and physical sciences of the National Institute, 6 Nivose, year 10.

Legendre, Méchain, and Prony, *Reporter.*

This report is approved, and its conclusions adopted by the class. Certified in conformity with the original by Delambre.

Paris, 26 Nivose, year 10 (16th January, 1802).

[The rest of this Paper is by the learned Secretary to the Royal Institution, Dr. Young.]

On examining the reduction of the standards of platina and iron to the freezing point, it appears that they differ somewhat less. Revision by Dr. Young.

less than is stated in the report, and that they coincide within an unit in the last place of the decimals expressing their magnitudes, or one ten thousandth of an inch. The standard of platina at the freezing point becomes equal to 39.37380, and that of iron to 39.37370 English inches on the scale of brass at 55° , and the mean of these to 39.37100 English inches at 62° , which is the temperature that has been universally employed in the comparison of British standards, and in the late trigonometrical operations in particular. This result agrees surprisingly with Mr. Bird's determination of the lengths of the toises sent by Mr. Lalande to Dr. Maskelyne, of which the mean was 76.734 inches: hence the metre, having been found to contain 36.9413 French inches, appears to be equal to 39.3702 English inches: or rather to be either 39.3694 or 39.3710, accordingly as the one or the other of the two toises happens to have been the more correct; we may therefore give the preference to that which measured 76.736 inches.

Allowing the accuracy of the French measurements of the arc of the meridian, the whole circumference of the globe will be 24855.43 English miles, and its mean diameter 7911.73. Taking the ellipticity at $\frac{1}{236}$, the axis will be nearly $7893\frac{1}{2}$, the equatorial diameter 7928, and the diameter of a sphere of equal solid content about 7916 miles; the brass standard being at the temperature of 62° of Fahrenheit.

Standard metre
at 62° .

As long, therefore, as the English standard continues to be reduced to this temperature, we must consider the metre as equivalent to 39.3710, and not to 39.3827 English inches.

Upon these joint authorities it may be of use to reprint here a table of the principal measures and weights now used in France, with the very slight corrections which this last comparison has introduced into it. In translating the French terms into English, we are fully at liberty to rescue them, in some measure, from the barbarisms in orthography which have been committed in forming them.

Measures of length, the metre being at 32° , the foot at 62° .

French measures of length,		English inches.	
		Millimetre	
	Millimetre	- - - - -	.03937
	Centimetre	- - - - -	.39371
	Decimetre	- - - - -	3.93710
			Metre

Metre	-	-	-	-	-	39.37100
Decametre	-	-	-	-	-	393.71000
Hecatometre	-	-	-	-	-	3937.10000
Chiliometre	-	-	-	-	-	39371.00000
Myriometre	-	-	-	-	-	393710.00000

	M.	F.	Y.	Ft.	In.
A decametre is	-	-	-	0	0 10 2 9.7
A hecatometre	-	-	-	0	0 109 1 1
A chiliometre	-	-	-	0	4 213 1 10.2
A myriometre	-	-	-	6	1 156 0 6

8 chiliometres are nearly 5 miles.

Measures of capacity.

	Cubic inches E.
Millilitre	.06103 of capacity.
Centilitre	.61028
Decilitre	6.10280
Litre, a cubic decimetre	61.02800
Decalitre	610.28000
Hecalitre	6102.80000
Chilolitre	61028.00000
Myriolitre	610280.00000

A litre is nearly $2\frac{1}{8}$ wine pints. 14 decilitres are nearly 3 wine pints. A chilolitre is 1 tun, 12.73 wine gallons.

Weights.

A gramme is the weight of a cubic centimetre of pure water at its maximum of density. It has been found equal to 18.827 French grains, of which 576 made 472.5 English; and 489.5058 grammes make a pound of the standard of the mint at Paris.

	E. grains.
Milligramme	.0154
Centigramme	.1544
Decigramme	1.5444
Gramme	15.4440
Decagramme	154.4402
Hecagramme	1544.4023
Chiliogramme	15444.0234
Myriogramme	154440.2344

A deca-

COMPARISON OF THE FRENCH METRE

decagramme is 6 dwts. 10.44 gr. tr. ; or dr. iif. gr. 4.44 ; or 5.65 dr. avoird. A hecatogramme is 3 oz. 8.5. A chiliogramme is 2lbs. 3 oz. 5 dr. av. A myriogramme is 22lbs. 1.15 oz. av. 100 myriogrammes are 1 ton nting 32.8lbs.

Agrarian measures.

in mea- Arc, 1 square decametre - - - 3,95 perches.
Hectare - - - 2 acres, 1 rood, 35.4 perches.

For fire-wood.

-wood. Decistere, $\frac{1}{10}$ stere - - - 3.5317 cub. f. E.
Stere, 1 cubic metre - - - 35.3171 cub. f.

MONEY. Copper.

		E. grains.
ey.	Centime, 1 gramme	15.4
	5 centimes, or fous	77.2
	Decime	154.4
	2 decimes	308.8

Silver $\frac{9}{10}$ or $\frac{15}{16}$ fine.

Franc, 5 grammes, - - - 5 dwts. 5.2 gr.
6 francs, - - - 16 dwts. 2.1 gr.

The franc is nearly the same with the livre tournois, and worth about 10d. Bolton's penny weighs 435 gr. ; his half-penny 165 ; a shilling nearly 93 gr. and is $\frac{11}{16}$ fine.

Length of
pendulum, &c.

It appears from Mr. Borda's experiments, that in latitude 45°, a pendulum of the length of a metre would perform in a vacuum 86116.5 vibrations in a day : the length of a pendulum being supposed to increase with the latitude, in the proportion of the square of the sine of the latitude, multiplied by .000567, while the time of its vibration remains unaltered.

Y.

VIII.

On the Figure of Sulphate of Barytes, and the Formation of Mandrepore. In a Letter from Mr. H. SARJEANT.

To Mr. NICHOLSON.

S I R,

IF the following mineralogical notices appear to contain any thing worthy of attention, they are at your service, from

Your humble servant,

Keswick, July 16, 1802.

H. SARJEANT.

THE barytes, or ponderous earth, occurs in a great variety of forms: one of the rarest is described as "resembling a number of small double convex lenses set edgewise in a ground." This singular formation, so different from the angular forms affected by crystals in general, was the cause of my examining it with considerable attention; and I am persuaded, that whoever does the same, will perceive that the shape of these crystals is not, properly speaking, lenticular, but rather a sort of very acute edged rhomboid, inserted by one of the corners as far as the diagonal line, so that the projecting part resembles the corner of a carpenter's chisel, admitting that the angle formed by its edge and side were enlarged to 100 degrees, or somewhat more, and the side bevilled off in the same manner as the edge, but in the opposite direction.

In its most usual form, the sulphate, the barytes is a very abundant production. It is found in this and the neighbouring counties, in great quantities, with lead, with lime (common limestone), with iron, and with gypsum.

That sort of limestone which abounds with the petrefactions called mandrepore, appears to owe its origin to depositions of calcareous mud, formed in a manner similar to what may be seen in the upper part of most lakes, that of Keswick in particular, in which a species of equisetum grows, often in sheets of several acres, slightly covered with water. The section of the mandrepore is precisely that of the fresh root, and they are generally inclined all in the same direction, as if caused by a stream of water passing over them, which might be the case when the water was drawn off from such places, by the effect of earthquakes, or other great operations of nature.

The subsequent induration of such mud into stone, may be considered as a fact sufficiently known.

IX. *Experiments*

IX.

Experiments and Observations on certain stony and metalline Substances, which at different Times are said to have fallen on the Earth; also on various Kinds of native Iron. By EDWARD HOWARD, Esq. F. R. S. From the Philosophical Transactions. 1802.

(Continued from page 221.)

Stone that fell in Yorkshire and was exhibited in London. 1796, weighing 56lb. Historical account.

IN 1796, a stone weighing 56lb. was exhibited in London, with several attestations of persons who, on the 13th of December, 1795, saw it fall, near Wold Cottage, in Yorkshire, at about three o'clock in the afternoon. It had penetrated through 12 inches of soil and 6 inches of solid chalk rock; and in burying itself, had thrown up an immense quantity of earth, to a great distance: as it fell, a number of explosions were heard, about as loud as pistols. In the adjacent villages, the sounds heard were taken for guns at sea; but, at two adjoining villages, were so distinct of something singular passing through the air, towards the habitation of Mr. Topham, that five or six people came up, to see if any thing extraordinary had happened to his house or grounds. When the stone was extracted, it was warm, smoked, and smelt very strongly of sulphur. Its course, as far as could be collected from different accounts, was from the south-west. The day was mild and hazy, a sort of weather very frequent in the Wold hills, when there are no winds or storms; but there was not any thunder or lightning the whole day. No such stone is known in the country. There was no eruption in the earth; and, from its form, it could not come from any building; and, as the day was not tempestuous, it did not seem probable that it could have been forced from any rocks, the nearest of which are those of Hamborough Head, at a distance of twelve miles.* The nearest volcano, I believe to be Hecla, in Iceland.

Investigations respecting it.

The exhibition of this stone, as a sort of show, did not tend to accredit the account of its descent, delivered in a hand-bill at the place of exhibition; much less could it contribute to re-

* Extracted from the printed paper delivered at the place of exhibition.

move the objections made to the fall of the stones presented to the Royal French Academy. But the Right Hon. president of the Royal Society, ever alive to the interest and promotion of science, observing the stone so exhibited to resemble a stone sent to him as one of those fallen at Sienna, could not be misled by prejudice; he obtained a piece of this extraordinary mass, and collected many references to descriptions of similar phenomena. At length, in 1799, an account of stones fallen in the East Indies was sent to the president, by John Lloyd Williams, Esq. which, by its unquestionable authenticity, and by the striking resemblance it bears to other accounts of fallen stones, must remove all prejudice. Mr. Williams has since drawn up the following more detailed narrative of facts.

Account of the Explosion of a Meteor, near Benares, in the East Indies; and of the falling of some Stones at the same Time, about 14 Miles from that City. By John Lloyd Williams, Esq. F. R. S.

Explosion of a meteor near Benares, and the falling of some stones at the same time.

A circumstance of so extraordinary a nature as the fall of stones from the heavens, could not fail to excite the wonder, and attract the attention of every inquisitive mind.

Among a superstitious people, any preternatural appearance is viewed with silent awe and reverence; attributing the causes to the will of the Supreme Being, they do not presume to judge the means by which they were produced, nor the purposes for which they were ordered; and we are naturally led to suspect the influence of prejudice and superstition, in their descriptions of such phenomena; my inquiries were therefore chiefly directed to the Europeans, who were but thinly dispersed about that part of the country.

The information I obtained was, that on the 19th of December, 1798, about eight o'clock in the evening, a very luminous meteor was observed in the heavens, by the inhabitants of Benares and the parts adjacent, in the form of a large ball of fire; that it was accompanied by a loud noise, resembling thunder; and that a number of stones were said to have fallen from it, near Krakhut, a village on the north side of the river Goomty, about 14 miles from the city of Benares.

Large ball of fire with noise like thunder.

The meteor appeared in the western part of the hemisphere, and was but a short time visible: it was observed by several Europeans, as well as natives, in different parts of the country.

In

In the neighbourhood of Juanpoor, about 12 miles from the spot where the stones are said to have fallen, it was very distinctly observed by several European gentlemen and ladies who described it as a large ball of fire, accompanied with a loud rumbling noise, not unlike an ill discharged platoon of musquetry. It was also seen, and the noise heard, by various persons at Benares. Mr. Davis observed the light come into the room where he was, through a glass window, so strongly as to project shadows, from the bars between the panes, on a dark coloured carpet, very distinctly; and it appeared to him as luminous as the brightest moonlight.

Degree of light, &c. When an account of the fall of the stones reached Benares, Mr. Davis, the judge and magistrate of the district, sent an intelligent person to make inquiry on the spot. When the person arrived at the village near which the stones were said to have fallen, the natives, in answer to his inquiries, told him, that they had either broken to pieces, or given away to the Tefieldar (native collector) and others, all that they had picked up; but that he might easily find some in the adjacent fields, where they would be readily discovered, (the crops being then not above two or three inches above the ground,) by observing where the earth appeared recently turned up. Following these directions, he found four, which he brought to Mr. Davis: most of these, the force of the fall had buried, according to a measure he produced, about six inches deep, in fields which seemed to have been recently watered; and it appeared, from the man's description, that they must have lain at the distance of about a hundred yards from each other.

Investigation on the spot by Mr. Davis. What he further learnt from the inhabitants of the village, concerning the phenomenon, was, that about eight o'clock in the evening, when retired to their habitations, they observed a very bright light, proceeding as from the sky, accompanied with a loud clap of thunder, which was immediately followed by the noise of heavy bodies falling in the vicinity. Uncertain whether some of their deities might not have been concerned in this occurrence, they did not venture out to enquire into it until the next morning; when the first circumstance which attracted their attention was, the appearance of the earth being turned up in different parts of their fields, as before mentioned, where, on examining, they found the stones.

Account by the natives.

The assistant to the collector of the district, Mr. Erskine, a very intelligent young gentleman, on seeing one of the stones, brought to him by the native superintendant of the collections, was also induced to send a person to that part of the country, to make inquiry; who returned with several of the stones, and brought an account similar to that given by the person sent by Mr. Davis, together with a confirmation of it from the Cauzy, (who had been directed to make the inquiry) under his hand and seal.

Inquiries by Mr. Erskine.

Mr. Maclane, a gentleman who resided very near the village of Krakhut, gave me part of a stone that had been brought to him the morning after the appearance of the phenomenon, by the watchman who was on duty at his house; this, he said, had fallen through the top of his hut, which was close by, and buried itself several inches in the floor, which was of consolidated earth. The stone must, by his account, previous to its having been broken, have weighed upwards of two pounds.

and Mr. Mac-lane.

At the time the meteor appeared, the sky was perfectly serene; not the smallest vestige of a cloud had been seen since the 11th of the month, nor were any observed for many days after.

State of the weather.

Of these stones, I have seen eight, nearly perfect, besides parts of several others, which had been broken by the possessors, to distribute among their friends. The form of the more perfect ones, appeared to be that of an irregular cube, rounded off at the edges; but the angles were to be observed on most of them. They were of various sizes, from about three to upwards of four inches in their largest diameter; one of them, measuring four inches and a quarter, weighed two pounds twelve ounces. In appearance, they were exactly similar: externally, they were covered with a hard black coat or incrustation, which in some parts had the appearance of varnish, or bitumen; and, on most of them were fractures, which, from their being covered with a matter similar to that of the coat, seemed to have been made in the fall, by the stones striking against each other, and to have passed through some medium, probably an intense heat, previous to their reaching the earth. Internally, they consisted of a number of small spherical bodies, of a slate colour, embedded in a whitish gritty substance, interspersed with bright shining spiculae, of a metallic or pyritical nature. The spherical bodies were much

Stones of this description which have been seen by the author.

Some account of the same.

harder than the rest of the stone: the white gritty part readily crumbled, on being rubbed with a hard body; and, on being broken, a quantity of it attached itself to the magnet, but more particularly the outside coat or crust, which appeared almost wholly attractable by it.

The author declines any conjecture.

As two of the more perfect stones which I had obtained, as well as parts of some others, have been examined by several gentlemen well versed in mineralogy and chemistry, I shall not attempt any further description of their constituent parts; nor shall I offer any conjecture respecting the formation of such singular productions, or even record those which I have heard of others, but leave the world to draw their own inferences from the facts above related. I shall only observe, that it is well known there are no volcanos on the continent of India; and, as far as I can learn, no stones have been met with in the earth, in that part of the world, which bear the smallest resemblance to those above described.

Iron in the Bornian collection said to have fallen in Bohemia.

IT remains for me to speak of a substance mentioned in the *Lithophylacium Bornianum*, Part I. page 125, described thus: "Ferrum refractorium, granulis nitentibus, matrice virescenti immixtis, (*Ferrum virens*, Linn.) cujus fragmenta, ab unius ad viginti usque librarum pondus, cortice nigro scoriaceo circumdata, ad Plann, prope Tabor, circuli Bechinensis Bohemiarum, passim reperiuntur."

The iron thus described, is moreover made remarkable by a note *, which observes, that credulous people assert it to have fallen from heaven during a thunder storm, on the 3d of July, 1753.

The collection of Baron Born, it is well known, has a place in the cabinet of the Right Honourable Charles Greville, who, from the effect produced by comparing the histories and structure of the Italian and Yorkshire stones with the description of this iron, was induced to search the collection of Born, where he discovered the very substance asserted to have fallen on the 3d of July, 1753. How far these four substances have resemblance to each other, it will soon appear not to be my province to anticipate.

* Quæ (fragmenta) 3 Julii, anni 1753, inter tonitrua, e cœlo pluviæ creduliores quidam asserunt.

The

The President having done me the honour to submit his specimens of the Yorkshire and Italian stones to my examination, I became indebted to Mr. Greville and Mr. Williams for a similar distinction: and, being thus possessed of four substances, to all of which the same origin had been attributed, the necessity of describing them mineralogically did not fail to present itself. To execute this task, no one could be more eager, and certainly no one better qualified, than the Count de Bournon. He has very obligingly favoured me with the following descriptions.

Other stones of similar origin.

Mineralogical Description of the various Stones said to have fallen upon the Earth. By the Count de BOURNON, F. R. S.

THE stones I am about to describe, are not of any regular shape; and those which were found in an entire state, that is, those which had not been broken, either by their fall or otherwise, were entirely covered with a black crust, the thickness of which was very inconsiderable.

Count Bournon's description of the various stones.

The stones which fell at Benares, are those of which the mineralogical characters are the most striking: I shall therefore begin the following description with them; and shall afterwards make use of them, as objects of comparison, in describing the others.

STONES FROM BENARES.

These stones, as well as the others described in this paper, whatever may be their size, are covered over the whole extent of their surface with a thin crust of a deep black colour: they have not the smallest gloss; and their surface is sprinkled over with small asperities, which cause it to feel, in some measure, like shagreen, or fish skin.

Stones from Benares. Thin black crust; rough like fish skin.

When these stones are broken, so as to shew their internal appearance, they are found to be of a greyish ash colour; and of a granulated texture, very similar to that of a coarse grit-stone: they appear evidently to be composed of four different substances, which may be easily distinguished, by making use of a lens.

Fracture; coarse grained, grayish ash colour; composed of four substances; viz.

One of these substances, which is in great abundance, appears in the form of small bodies, some of which are perfectly globular, others rather elongated or elliptical. They are of various sizes, from that of a small pin's head to that of a pea, or nearly

Opaque gray globules, of conchoidal fracture and slight lustre, slightly giving fire with steel, so: and abrading glass;

so: some of them, however, but very few, are of a larger size. The colour of these small globules is gray, sometimes inclining very much to brown: and they are completely opaque. They may, with great ease, be broken in all directions: their fracture is conchoid, and shews a fine, smooth, compact grain, having a small degree of lustre, resembling in some measure that of enamel. Their hardness is such, that, being rubbed upon glass, they act upon it in a slight degree; this action is sufficient to take off its polish, but not to cut it: they give faint sparks, when struck with steel.

Pyrites, not
magnetical.

Another of these substances, is a martial pyrites, of an indeterminate form: its colour is a reddish yellow, slightly inclining to the colour of nickel, or to that of artificial pyrites. The texture of this substance is granulated, and not very strongly connected: when powdered, it is of a black colour. This pyrites is not attractable by the magnet; and is irregularly distributed through the substance of the stone.

Metallic malle-
able iron.

The third of these substances consists in small particles of iron, in a perfectly metallic state, so that they may easily be flattened or extended, by means of a hammer. These particles give to the whole mass of the stone, the property of being attractable by the magnet; they are, however, in less proportion than those of pyrites just mentioned. When a piece of the stone was powdered, and the particles of iron separated from it, as accurately as possible, by means of a magnet, they appeared to compose about $\frac{1}{15}$ of the whole weight of the stone.

Whitish gray,
crumbly earth.

The three substances just described, are united together by means of a fourth, which is nearly of an earthy consistence. For this reason, it is easy to separate, with the point of a knife, or even with the nail, the little globular bodies above mentioned, or any other of the constituent parts of the stone you may wish to obtain. Indeed the stone itself may readily be broken, merely by the action of the fingers. The colour of this fourth substance, which serves as a kind of cement to unite the others, is a whitish gray.

The crust is
probably black
oxide of iron.

The black crust with which the surface of the stone is coated, although it is of no great thickness, emits bright sparks, when struck with steel: it may be broken by a stroke with a hammer; and seems to possess the same properties as the very attractable black oxide of iron. This crust is, however, like the substance of the stone, here and there mixed with small particles of iron

In the metallic state: they may easily be made visible, by passing a file over the crust, as they then become evident, on account of their metallic lustre. This is more particularly the case with respect to the crust of those stones which remain to be mentioned, they being much more rich in iron than that I have just described; a circumstance I think it needless to repeat, in the following descriptions of them. The stone now treated of, does not, when breathed upon, emit an argillaceous smell: the same remark may be applied to all the others.

The specific gravity of this stone is 3352.

STONE FROM YORKSHIRE.

This stone, the constituent parts of which are exactly the same as those of the stones from Benares, differs from them, however,

First. In having a finer grain.

Secondly. That the substance described as being in the form of small globular or elliptical bodies, is not so constantly in those forms, but is also found in particles of an irregular shape; a circumstance that is not met with in the other stones: these bodies are likewise, in general, of a smaller size.

Thirdly. The proportion of martial pyrites, which has precisely the same characters as that in the stones from Benares, is less; on the contrary, that of the iron in a metallic state, is much greater. The quantity I was able to separate by means of the magnet, appeared to me to compose about eight or nine parts, in one hundred, of the weight of the whole mass. I observed many pieces of this iron, of a pretty considerable size; one of them, taken from a portion of the stone I had powdered, in order to separate the iron, weighed several grains.

The part of the stone which is in an earthy state, and which serves to connect the other parts together, has rather more consistence than that of the preceding stones; and its appearance does not differ much from that of decomposed felspar or kaolin. The stone itself, therefore, although by no means hard, is rather more difficult to break with the fingers.

The specific gravity of this stone is 3508.

Stone from
Yorkshire.
Composed like
the former;
but of finer
grain:
The globules are
less regular.

The pyrites in
less quantity;
and the metallic
iron more.

Its earthy part is
more consistent.

It is denser.

STONE FROM ITALY.

This stone was in a perfectly entire state; consequently, its whole surface was covered over with a black crust peculiar to all

Stone from Italy.

all stones of this kind. As the stone was of a very small size, it became necessary to sacrifice the whole of it to the investigation of its nature. Its grain was coarse, similar to that of the stones from Benares: in it might be perceived the same gray globular bodies, the same kind of martial pyrites, and the same particles of iron in the metallic state. The proportion of these last was much less than in the stone from Yorkshire; but rather greater than in the stones from Benares. The same kind of gray earthy substance served to connect the different parts together; and nothing more could be perceived, except a few globules, which consisted wholly of black oxide of iron, attractable by the magnet, and one single globule of another substance, which appeared to differ from all those we have already described. This last substance had a perfectly vitreous lustre, and was completely transparent: it was of a pale yellow colour, slightly inclining to green; and its hardness was rather inferior to that of calcareous spar. The quantity of it, however, was too small to be submitted to such an investigation as might have determined its nature. The black crust which covered the stone, was rather thinner than that of the stones already described; and seemed to have undergone a kind of contraction, which had produced in it a number of fissures or furrows, thereby tracing upon the surface the appearance of compartments, similar in some measure to what is observed in the stones called Septaria.

The specific gravity of this stone was 3.418.

STONE FROM BOHEMIA.

Stone from Bohemia. Composed like the preceding;

The internal structure of this stone is very similar to that of the stone from Yorkshire. Its grain is finer than that of the stones from Benares: in it may be observed the same gray substance, both in small globules and in particles of an irregular shape; also the same particles of metallic iron. The same kind of earthy substance likewise served to connect the other parts together.

This stone, however, differs materially from the others.

but its pyrites very minute; the quantity of metallic iron much greater;

First. The particles of pyrites cannot be seen without a lens. Secondly. It contains a much larger quantity of iron in the metallic state; inasmuch, that the proportion of that metal, separated from it by means of the magnet, amounted to about $\frac{2}{100}$ of the weight of the whole.

This

This stone has also (owing perhaps to its having remained a ^{and more oxidized} much longer time in the earth than the preceding ones, all of which were taken up nearly at the very instant of their fall) another difference, viz. many of the particles of iron in a metallic state, have undergone an oxidizement at their surface; a circumstance that has produced a great number of spots, of a yellowish brown colour, and very near to each other, over a part of its internal substance. This oxidizement, by adding to the bulk, and to the force of action, of the part we have described as serving by way of cement to the other constituent parts of the stone, has occasioned a greater degree of adhesion between these parts, and has rendered the substance of the stone more compact.

The great quantity of iron in a metallic state which this ^{It is harder, and} stone contains, added to its greater compactness, makes it ^{admits a slight} capable of receiving a slight degree of polish; whereas it is im- ^{polish.} possible to give any polish to the others. When polished, the iron becomes very evident, in the polished part; appearing in the form of small specks, almost close to each other, which have the colour and lustre peculiar to that metal: these specks are, in general, nearly of an equal size.

The black crust of this stone is similar to that of the others.

The specific gravity of the stone is 4281.

It is easy to perceive, from the foregoing description, that ^{These stones are} these stones, although they have not the smallest analogy ^{not in the small-} with any of the mineral substances already known, either of a ^{est degree analo-} volcanic or any other nature, have a very peculiar and striking ^{gous with any} analogy with each other. This circumstance renders them ^{other known} truly worthy to engage the attention of philosophers; and ^{minerals,} naturally excites a desire of knowing to what causes they owe their existence.

(To be continued.)

Page 88

X.

An Answer to Mr. Gough's Essay on the Theory of Compound Sounds. By THOMAS YOUNG, M. D. F. R. S.

To Mr. NICHOLSON.

S I R,

Introductory remarks.

I HAVE already made public, through the medium of your useful Journal, one vindication of my remarks upon Dr. Smith's Harmonics; and I shall now beg you to insert some further observations upon a subject nearly similar. I do not think myself bound to reply to all the arguments that may happen to be advanced against any of my opinions; but when they come from persons of literary respectability, and especially when they convey an imputation of having detracted from the merits of others, I cannot forbear attempting to answer them; and in the present instance a very few words will be sufficient for the purpose. Mr. Gough has published in the last volume of the Manchester Memoirs, a paper in which he professes to defend Dr. Smith's opinions against mine: but he has unfortunately so far mistaken the question between us, that with respect to the principal circumstances, while he imagines he is confuting me, he is completely on my side.

The author's inferences from coalescence of undulations as to the doctrines of sound and light, scarcely to be called a new theory.

Proceeding upon the grounds of the well known facts relative to the grave harmonics, or the third sounds produced by the coalescence of two others, as well as upon the allowed principles of the composition of motion, I had drawn a number of inferences, which appeared to be of some importance in the first place with respect to the doctrine of sound, and which have since furnished me with still more interesting conclusions with regard to light. I was not aware that there was any probability of the justice of my inferences being disputed, or that there was so much novelty in the mode of obtaining them, as to deserve the name of a theory or an invention: but if it be deemed otherwise, I shall always be ready to acknowledge the invention with satisfaction, and to support the theory with alacrity.

Dr. Smith imagined that, when undulations in

I shall now quote the words of Dr. Smith, which gave rise to my animadversions; and this statement is the more necessary,

fary, as I before omitted to detail the particulars of that author's misconception, and as Mr. Gough appears wholly to have overlooked the passage. "Different particles of the air at the ear" says Dr. Smith, "will keep moving constantly opposite ways at the same time, And in so rare a fluid as air is, where the intervals of the particles are eight or nine times greater than their diameters, there seems to be room enough for such opposite motions without impediment: especially as we see the like motions are really performed in water, which in an equal space contains eight or nine hundred times as many such particles as air does. For when it rains upon stagnating water, the circular waves propagated from different centres, appear to intersect, and pass through, or over each other, even in opposite directions, without any visible alteration in their circular figure, and therefore without any sensible alteration of their motions." *Harmonics*, 1759, p. 105.

air or in water cross each other, different particles of the fluid were at the same time moved *inter se*, according to the respective undulations.

It certainly would have "cost me an effort of study" to demonstrate this, although I could not exactly consider it as the "intuitive conclusion of a comprehensive mind." Such a mind appeared to me to comprehend with equal ease the distinct and the confused; and I am only at a loss to conceive how the writer of this passage could ever have composed a valuable treatise on optics. Mr. Gough's theory differs as widely from this statement as mine does. My remark on it was this; "It is surprising that so great a mathematician as Dr. Smith could for a moment have entertained an idea, that the vibratious constituting different sounds, should be able to cross each other in all directions, without affecting the same individual particles of air by their joint forces: undoubtedly they cross, without disturbing each other's progress; but this can be no otherwise effected than by each particle's partaking of both motions. If this assertion stood in need of any proof, it might be amply furnished by the phenomena of beats, and of the grave harmonics observed by Romieu and Tartini, which Mr. Lagrange has already considered in the same point of view." *Phil. Trans.* 1800, p. 130.

Whereas the same individual particles of the fluid receive and transmit both undulations.

I have no objection to admitting the whole of Mr. Gough's propositions, in the particular cases which he has considered; but when he says that the coalescence of two sounds is impossible on mechanical principles, he thinks proper to omit the only case in which I had asserted its existence, that is, when

Sounds may coalesce which arrive at the ear in the same direction;

the sounds arrive at the ear "in the same direction." p. 131. Surely Mr. Gough will not deny the possibility of such a coincidence in direction, so far at least that the physical effect may be the same as if the coincidence were perfect: when, for instance, we listen to two or more sounds passing through a long tube, or to the various subordinate sounds of the same chord or pipe. If he be actually disposed to deny the perfect coincidence in practical cases, I shall only appeal to experience, which shows that the effect of a third sound is most distinctly heard, whenever the theory leads us to expect it; but there will always be a much greater portion of each sound so reflected from the surrounding objects as not to coincide in direction sufficiently for coalescence, and hence the original notes will always be much more audible than the new compound. The ear indeed appears to have greater powers of analysis than one would naturally have expected, it decomposes a "compound" just as if it were a mere "mixture," not only in this case but in many others: how it performs this operation, I do not pretend to determine. When Mr. Gough represents me as maintaining that two musical strings, differing in the times of their vibration, and happening to vibrate in concert, do not occasion two distinct sounds, and that the waves of air are compelled by their mutual interference to coalesce, thereby producing a new succession of pulses, constituting a single sound in the place of the former; his expressions tend to impute to me an opinion which could only be maintained by a person who had never heard a single musical composition, or ever been present at the conversation of a mixed company.

not by losing
their respective
tones in one sin-
gle sound; but
by producing an
additional weak-
er sound.

All imperfect
unisons must
produce beats.

I am not solicitous for the application of the term compound by coalescence, to the human voice; but Mr. Gough can scarcely form to himself a distinct conception of it, very different from mine. A mixture of imperfect unisons would inevitably be accompanied by the production of beats; and if he assert that the imperfection is too small to produce this effect, I will only request him to assign any reasonable limit to its magnitude, and by producing the note long enough, I will show that a beat must necessarily ensue. Perhaps a wish to retain the Newtonian theory of the law of the undulations may have led him into these superfluous refinements.

H

If Mr. Gough will take the pains of examining the phenomena of the grave harmonics, which he seems to have hitherto thought beneath his notice, he will be convinced that the coalescence of musical sounds is not only possible, but of very frequent occurrence, and that the compound sound does actually "acquire sensible properties peculiar to itself;" and if he will explain, in any other manner than I have done, the indubitable fact of the audible impression of the presence of the fourth below the key, in consequence of the coexistence of the sounds constituting the interval of a major third, even when both the notes are freed from their harmonics, and when all echo is avoided; I shall then listen to him, with the attention due to a person who endeavours to regulate his arguments by the results of accurate observation.

The phenomena of the grave harmonics shew that musical sounds coalesce in numerous instances,

I am, Sir,

with great respect,

Your obedient humble Servant,

THOMAS YOUNG.

Royal Institution, May 30, 1802.

CORRECTION.

In your fifth volume (quarto) p. 166, l. 20, *for* "a better third than that," *read*, "equal to the third."

The following CORRECTIONS made by Dr. Young in his Paper on the Mechanism of the Eye, which is inserted in our fifth volume (Quarto Series) were omitted to be made in their proper places.

Page 256, line 6, Prop. III. *after e*, insert the base being unity.

Page 257, line 15, Cor. 10. *for* ntu , *read* ntt ; line 16, *for* product &c. *read* square of the cosine of incidence.

Page 258, line 2, Cor. 11. *for* $1 + u^2 - 2u^2$, *read* $2muu$.

Page 258, Prop. V. Cor. See the note in p. 299.

Page 259, Prop. VIII. By a mistake of a sign, the eighth proposition is rendered erroneous; no use having been made of that proposition, it has been inserted without proper revision. It ought to stand thus, with its demonstration:

PROPOSITION VIII. PROBLEM.

To find the path of a ray of light falling obliquely on a sphere, of a refractive density varying as any power of the distance from the centre.

The

The refractive density, in the case of these propositions, varies as the ratio of the sines, and as the velocity of light in the medium.

(Schol. 2. Prop. I.) Let the velocity at the distance x be $x^{-\frac{1}{r}}$; then, considering the refractive force as a species of attraction, we have, in Prop. 41. 1. 1. Princip. $\sqrt{ABFD} = x^{-\frac{1}{r}}$, $Q = s$, the sine of incidence, the radius being unity, $Z = sx^{-\frac{1}{r}}$, $Dc =$

$$2xx\sqrt{x^{-\frac{2}{r}} - s^2x^{-2}} = \frac{1}{2}sx^{\frac{1}{r}-2} \cdot 1 - s^2x^{\frac{2}{r}-2} \Big)^{-\frac{1}{2}},$$
 and
 the fluxion of the area described by the radius $= -\frac{1}{2}sx^{\frac{1}{r}-3}$

$\cdot 1 - s^2x^{\frac{2}{r}-2} \Big)^{-\frac{1}{2}}$. Let the sine of the inclination to the radius at each point be called y ; then $y = sx^{\frac{1}{r}-1}$, $\dot{y} = \frac{1-s}{r}sx^{\frac{1}{r}-2}$, \dot{x} , and the fluxion of the area $= \frac{r}{2r-2} \dot{y} \cdot 1 - yy \Big)^{-\frac{1}{2}}$, of which the fluent is $\frac{r}{2r-2} Y$, y being the sine of the arc Y ; and the angle corresponding is $\frac{r}{r-1} Y$. The value of that angle being found for

any two values of x or y , the difference is the intervening angle described by the radius. This angle is therefore always to the difference of the inclinations as r to $r-1$, and the deviations is to that difference as 1 to $r-1$.

Corollary. Hence, in the passage to the apsis, and the return to the surface, the deviation is always proportionate to the arc cut off by the incident ray produced: therefore such a sphere could never collect parallel rays to any focus, the lateral density being too small towards the surface.

Page 259, line 9, *for* but the two last, &c. *read* the seventh may either be deduced from the eighth, or may be demonstrated independently of it.

Page 264, last line, *after* internally, *insert* Or, if a lens of equal mean dimensions, and equal focal length, with the crystalline, be supposed to consist of two segments of the external portion of such a sphere, the refractive density at the centre of this lens must be as 18 to 17.

XI. Experiments

XI.

Experiments on the Velocity of Air issuing out of a Vessel in different Circumstances; with the Description of an Instrument to measure the Force of the Blast in Bellows, &c. By Mr. BANKS, Lecturer in Natural Philosophy.*

THE object of this inquiry may be announced in the following proposition: If an elastic fluid is generated in a given vessel, or any way contained in it, and at liberty to issue out of the said vessel through a given aperture, to determine the resistance which the vessel meets with from its action, or the power which it has of communicating motion to the vessel, as in a sky-rocket, Sadler's steam engine, &c.

Proposition. To determine the mechanic force or reaction of an elastic spouting fluid.

Before we proceed to relate the experiments, it may be proper to premise certain principles deduced from theory. If a tube be filled with any kind of fluid, as air, water, mercury, &c. and placed in a vacuum, every fluid will flow out with the same velocity. For though the pressure of a column of mercury of a given altitude be much greater than an equal column of water, yet the weight of the particles to be projected is greater in the same ratio. On the other hand, if air is lighter than water, the particles projected are also lighter in proportion. If a tube of 16 feet high be filled with air of any density, that air, like water, would flow into a vacuum with a velocity of 32 feet per second, no corrections being made for resistance †.

All uniform fluids flow from an orifice with the same velocity in vacuum, if their heights be equal, however different their densities.

And if we take the gravity of air to water as 1 to 840, then a column of one foot of water compressing air, will produce as great a velocity in that air as a column of air 840 feet high, supposing it was of uniform density.

Equal velocities will be produced in one issuing fluid by the pressure of another; if the height of the former to that of the latter, be inversely as its density.

If we take the whole pressure of the atmosphere equal to 53 feet of water, or its height (supposing it to be equally dense,

* From the Manchester Memoirs, Vol. V. p. 398.

† In the supposition of a perpendicular tube open at the top, filled with air or any elastic fluid, the author takes the density of the column at the bottom, or where the aperture is made, to arise solely from the weight of the elastic column; and the altitude to be that which would be if the whole column were reduced to the density of that at the bottom,

which

The atmosphere which in this case will make no difference) equal to 33 multiples equal to 33 feet of water, or 27720 feet of air. It would therefore propel air into a vacuum with the velocity due to this fall: viz. $\sqrt{16} : \sqrt{27720}$ as 32 feet : 1332 feet per second.

To prove whether air compressed by 33 feet of water would be impelled into the atmosphere with the above velocity, I have made, amongst many more, the following experiments :

Experiments. *A*, plate xiv. fig. 1 *, is a vessel of a known capacity, into the top of which is screwed an aperture of a known area. The tube *Td*, recurve at *d*, is foldered or screwed into the top of the said vessel. The hole *a* is stopped, and water poured into the tube at *T* till it is full, at which time a quantity of water will have passed out of the tube at *d*, and condensed the air in the vessel, more or less as the tube *Td* is longer or shorter.

At this time a person who has closed the aperture at *a* with a finger of one hand, and held a half second pendulum in the other, removes both at the same time, while at the same moment an assistant opens a cock over the tube *T*, which supplies it with water as fast as it can descend into *A*. The moment that the water appears at *a*, the time-piece is stopped, and the time of expelling the air is noted, from which, by knowing the capacity of the vessel, the velocity may be obtained.

If the tube *Td* should be continued near the bottom of the vessel *A* while it was filling with water, the length of the compressing column would be gradually diminishing, and of consequence the pressure would be constantly changing; hence the open end of the tube is as near the top of the vessel as is consistent with a free passage for the water.

EXPERIMENTS.

The vessel *A* contained 15 lb. 6 oz. of water, from which we find its capacity is 425.088 cubic inches.

The area of the aperture *a*, through which the air is expelled, .0046 inches.

* There being no figure in the Transactions, I have inserted the drawing referred to. N.

Exper.

Exper. I. The altitude of *T* above the vessel is 30 inches. Experiment I.
Time of expelling the air, by several trials, is 33 seconds. with a fall of 33 inches.

Exper. II. The altitude of *T* is six feet. The time of filling, Experiment II.
by several trials, is 21.3 seconds. with a fall of 72 inches.

In the first experiment, 425.088, the solidity of the vessel, divided by .0046, the area of the hole *a*, gives 92410.4 inches for the length of the stream of air driven out in 33 seconds; divide that length by 33, and we shall have 233.3 feet, the velocity per second, communicated by 30 inches of water. Computation of the first experiment gives actual velocity per sec. = 233.3 feet;

The second experiment by the same process gives 361.6 feet per second. and of the second gives 361.6 per second. If we would compare these together, we may say, as the square root of 30, the head, is to 233.3, the velocity; so is the square root of 72, the second head, to 361.8 feet, the velocity per second.

Again:—As the square root of 6 feet is to 361.6, so is the square root of 33 feet to 845.2 feet per second, the velocity produced by that head, or the initial velocity with which the atmosphere would enter a vacuum. This velocity, found by experiment, is 487 feet per second less than has been assigned by theory. Whence the velocity of air from the atmosphere into a vacuum is computed, and turns out to be 845.2 instead of 1332 feet, as by theory.

It appears however that the results, as determined by theory and experiment, do not differ more than in the case of effluent water. For, if we would reduce the velocity of effluent water found by theory, to that which experience gives, we must multiply it by .634. Accordingly, if we multiply 1332 feet, the velocity of the atmosphere entering a vacuum as calculated above, by .634, the product is 844.5 per second, differing but $\frac{2}{10}$ of a foot from that just found by experiment. But this result, corrected by the multiplier given by experience for effluent water, proves to be very correct.

I have also made experiments by sinking vessels in water, till their tops were even with its surface, and opening the aperture that the rising water might expel the air, by which I obtained the same velocities as above; but the method of computing is much more intricate, for which reason I shall not insert them. Experiments with rising water.

From the above it appears, that a pressure equal to 33 feet of water, will expel air out of the bellows into the atmosphere with a velocity of 845 feet per second; that one foot of water in depth will produce a velocity of $147\frac{1}{2}$ feet, and one inch a velocity of 42 feet per second, or 20 miles an hour. General result:

Hence we may construct a table shewing the velocity communicated to air by any head of water; for as the square root applied to the construction of tables for effluent air pressed by water;

of 6 feet is to the velocity produced by that head, so is the square root of any other depth to the velocity produced by that depth.

or in bellows,

We may also, from the above, construct an instrument which will shew the velocity with which air flows out of any kind of bellows, with as much accuracy, as the experiments have been made on which its construction depends.

Description of the Instrument, &c.

by means of an instrument here described. It is a portable vessel containing water, to be formed into an open gage tube;

The metal box or tube *B*, plate xiv. fig. 2, may be about the size of the figure; the top must be made air tight by the cover *L*; into the bottom is fixed the small tube *AC*, and into the piece *D* is cemented the glass tube *ED*; the instrument is then inverted, and some water poured through the tube *AC*, till, when in its proper position, it is visible at *D*. It is now ready for use, and the end *A* may be fixed in a hole made in the upper board of the bellows, and the water will rise in the glass tube, in smith's bellows, generally from 9 to 12 inches, furnace bellows, generally four feet or more. But where the compression is great, quicksilver may be used instead of water, only in this case the instrument should be made of iron, as quicksilver causes the screws of brass to break. Or, instead of quicksilver, the tube *ED* may be sealed at the top *E*, and then a length of 12 or 18 inches will be enough for any blast. The glass tube needs not be more than one-eighth or one-tenth of an inch in diameter.

Whatever compression there may be in the bellows, there will be the same in the upper part of the tube *B*, which will force the water into the glass tube *DE*, and make the air in its upper part of the same density, deducting from the compressing force the altitude of the water raised above *D*, which however will be of little or no importance; if the gauge is placed in a horizontal position, with the glass tube downward, there will be no difference of density.

or the tube may be hermetically closed.

The computation for the force in the case where a tube hermetically sealed at the top is adopted in the instrument, will be effected by considering that the space occupied by any elastic fluid is inversely as its force. Thus, let the tube be 12 inches long, and suppose the water to be raised one inch, then it will be $11 : 12 ::$ the force of the atmosphere : the force of the air in the tube $:: 1 : 1\frac{1}{11}$. Hence a scale may be adapted to the instrument,

instrument, to express the force of condensation over and above the common atmospheric pressure; which force is signified in the instance above, by the fraction $\frac{1}{11}$, unity being the atmospheric pressure. If we denote the atmospheric pressure by 30 inches of mercury, or 32 feet of water, then the force $\frac{1}{11}$ in the above example, will be expressed by 2.727 inches of mercury, or 2.91 feet of water; and the like for any other instance.

If a mercurial instrument of the above construction be preferred, it becomes necessary to add the height of the mercurial column to the force found as above: thus, if the condensation of air be from 12 into 9 inches, then the addition to the force of the internal air in the tube is equal to $\frac{1}{3}$, or 10 inches of mercury, to which must be added the three inches raised in the tube, and the whole force will be 13 inches of mercury, exclusive of that of the atmosphere.

If mercury be used, its density must be considered in the short instrument.

This sort of instrument or gauge serves equally well for finding the expansive force of any kind of elastic fluid, as for measuring the velocities with which they issue out of the place of their confinement. It may be applied to all kinds of bellows, to condensed steam, and to the air pump.

General application to all gases.

XII.

On the Variation of Rate in a Time-Piece, as indicated by the Changes in the Arc of Vibration. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR,

Lynn, July 20, 1802.

FROM what I have already mentioned in a former paper *, it follows, that in clocks used in making astronomical observations, it is necessary to observe the arc of vibration very frequently, and when it is found different from that generally described by the pendulum, the rate of the clock must on this account be corrected.

* Expediency of frequent observation of the arc of vibration in clocks.

To determine this correction, let x denote the time which a pendulum, vibrating in an arc exceedingly small, will lose when made to vibrate in a larger arc of the same circle, T the

Rule for deducing the variation in time from that of the arc.

* See pa. 76 of this vol.

VARIATION OF RATE IN A TIME-PIECE,

of seconds in 24 hours (86400), and D the number of degrees described on each side of the perpendicular *.

$x = T \times \frac{D^2}{52480}$ nearly. Consequently $x = D^2 \times$ nearly.

From the above theorem the following table was computed, which shows the time lost by a pendulum in 24 hours, by its semi-arc of vibration in the same circle 1° of a

Table of
times
lost
by
pendulums
in
their
vibrations

Half arc of 1 of a degree	Losses per day of a pendulum vi- brating the last	Difference.	Losses per day on increasing its se- mi-arc of vibra- tion 1° of a degree.
	0, 0		
0 $\frac{1}{4}$	0, 1	0, 1	, 006
0 $\frac{1}{2}$	0, 4	0, 3	, 020
0 $\frac{3}{4}$	0, 9	0, 5	, 033
1	1, 6	0, 7	, 046
		0, 9	, 060
1 $\frac{1}{4}$	2, 5	1, 2	, 080
1 $\frac{1}{2}$	3, 7	1, 3	, 086
1 $\frac{3}{4}$	5, 0	1, 6	, 107
2	6, 6	1, 7	, 113
2 $\frac{1}{4}$	8, 3	2, 0	, 133
2 $\frac{1}{2}$	10, 3	2, 1	, 140
2 $\frac{3}{4}$	12, 4	2, 4	, 160
3	14, 8	2, 6	, 173
3 $\frac{1}{4}$	17, 4	2, 8	, 187
3 $\frac{1}{2}$	20, 2	3, 0	, 200
3 $\frac{3}{4}$	23, 2	3, 2	, 213
4	26, 3		

* Simpson's Fluxions, Art. 464.

The

The use of this table will be easily understood by the following examples.

EXAMPLE I.

Suppose a clock goes mean solar time when the pendulum vibrates 2° on each side of the perpendicular, what will it lose per day when the pendulum vibrates through an arc of three degrees?

Against 2° and 3° in the first column, we have 6,"6 and 14,"8 respectively in the second column. Then $14,"8 - 6,"6 = 8,"2 =$ the time lost per day.

EXAMPLE II.

Suppose a clock gains $1''$ per day, when the pendulum vibrates $1^{\circ} 49'$ on each side of the perpendicular, what would be its rate when the pendulum vibrates $1^{\circ} 51'$?

Between $1^{\circ} \frac{3}{4}$ and 2° in the first column, we have ,,"107 in the fourth column. Then $1'' - ,,"107 \times 2 = 0,"786 =$ the daily rate of the clock gaining.

I am, with respect,

Sir,

Your humble servant,

E. WALKER.

ANNOTATION.

The arc of vibration in clocks is likely to be affected in practice by changes in the density of the air, in its temperature, and in the action of the first mover; and these causes will most probably afford results considerably different from those inferred from the simple pendulum in vacuo. I take it to be experimentally ascertained by the registers of the performance of astronomical clocks very firmly fixed, that the gridiron pendulum, with a very flexible spring suspension, is not sensibly affected during the different stations of the barometer and thermometer; and I apprehend that the first mover, if a weight, must operate either with a constant force or with periodical variations, occasioned by the train or mechanism through which its action is transmitted, the influence of oil, &c. which periods are rendered shorter by the well known methods of disengaging the escapement, or parts nearest the pendulum. Many other considerations will offer themselves to the learned author, as to

Whether theory can be applied to an engine so compounded as a clock, &c.

those parts of the vibration to which the maintaining power may be applied so as to increase the arc, while the time may be either lengthened or shortened according to the circumstances, the general nature of the escapement, &c. and these will probably lead to a conclusion, that the corrections of time to be applied to a clock, which either in its arc of vibration or otherwise gives symptoms of irregularity, can be gained only from direct observation.

W. N.

XIII.

Description of Atkins's Hydrometer for ascertaining the specific Gravities of spirituous Liquors. By J. FLETCHER, Esq. Communicated by the Author.

Utility of descriptions of philosophical instruments.

AMONGST the various papers which are to be found in the works of those journalists who have so materially contributed to the advancement of our knowledge, there are perhaps but few which have been more effectually conducive to this end than those which are appropriated to the description of the instruments of science. It is indeed much to be lamented, that the merchant, the manufacturer, and the artizan, should continue to be so generally deprived of the advantages to be obtained from the use of such of them as are adapted to their purposes, for want of the necessary information with regard to the mode of applying them.

The instrument which is here intended to be described, is one of those which stand in a great measure in this predicament; and as it appears very well to deserve a description, it is conceived that, imperfect as the following one is, it will not be unacceptable.

Spec. gr. only criterion of the strength of spirits.

It is now universally acknowledged, that the specific gravities of spirituous liquors afford the only tolerable criterion of their strength, and consequently of their comparative values. To ascertain the specific gravity of a liquid with a considerable degree of precision, is an operation of no great difficulty. The most accurate method of performing it is perhaps that which is also the simplest: to weigh the fluid in a vessel in which its bulk is capable of being nicely measured. Much more, however,

ever, remains to be done in order to the estimation of the strengths of spirituous liquors: the contraction of the mass of a compound of alcohol and water on their mixture, and the variation in its measure in respect of temperature, are each of them of sufficient practical importance to render their appreciation necessary when the value of the spirit is to be discovered. Thus for example, 18 gallons each of alcohol and water will produce only 35 gallons of the compound, and a difference of 30° in temperature of Fahrenheit's scale, occasions such a change in the specific gravity of proof spirit, as, if omitted to be taken into consideration, would render the dealer liable to an error of upwards of ten per cent. in the estimation of its strength and consequent value.

In commercial transactions with respect to spirituous liquors, it is necessary to appreciate their strength by comparing it with that of spirit of a certain supposed quality in this respect as a standard. This standard-spirit (which is called *proof-spirit*) is of such a degree of strength that its specific gravity is about .920 at 60° of Fahrenheit's thermometer; and the object of enquiry in all cases is with regard to the quantity of this proof spirit which would be equivalent to a given quantity of any spirit under examination. The language of the spirit-dealers with respect to their terms of "*over-proof*" and "*under-proof*," has in all cases this kind of reference to commercial value.

When they say that a certain kind of spirit is 30 per cent. over proof, they mean that if 100 parts by measure be increased to 130 by the addition of water, it will become of proof strength; and when they say that it is 30 per cent. under proof, they mean that 70 parts by measure of proof-spirit will become, when increased to 100 by the addition of water, of equal strength to that of the spirit in question. If therefore a gallon of proof spirit be worth 8s. 4d. the same quantity of the former kind of spirit will be worth 10s. 10d. and of the latter 5s. 10d.

It is not therefore sufficient for the purchaser of a sample of spirit, or the officer who is to collect the duties on it, to be informed of its specific gravity at a given temperature: it is necessary for him to ascertain the quantity of proof-spirit which is capable of producing, or being produced, by the spirit in question by the addition of water only, and which is consequently equivalent to it in value.

Corrections necessary to be applied for concentration and temperature.

18 parts each of alc. and water produce only 35 of the mixture. A variation of 30° of temp. occasions a change of 10 per cent. in the apparent strength of proof-spirit. Commercial estimation of value of spirituous compounds,

by comparison with proof-spirit. Definition of terms "over-proof" and "under-proof."

The desideratum is to find the equivalent quantity of proof-spirit.

The

- Calculation in-
applicable to this
purpose.
- Inconvenience
of tables.
- Instrumental
solution prefer-
able.
- The hydrometer
with many
weights,
- with a sliding-
rule.
- The latter most
eligible.
- Description of
Atkins's hydro-
meter.
- Square stem,
with the letters
of the alphabet
on one face,
- and four
weights.
- When it emerges
to the bulb with
one weight, the
next heavier
sinks it to the
top of the stem.
- Stem and divi-
sions thus in
effect multiplied
by 5.
- With the
weights singly
applied, indicates
specific gravities
from .806 to
1.000;
- The solution of this problem being almost impossible by any application of mathematical formulæ, it is of course desirable to be enabled to obtain it instrumentally, or by inspection of tables previously laid down from experiment; and the use of the latter, under such circumstances as those in which operations of this kind are generally performed, having been found inconvenient, its instrumental solution has been universally preferred. This has been attempted principally in two ways: by the hydrometer, with a multiplicity of weights adapted to the various corrections for temperature; and by the same instrument more simply constructed, so as to indicate merely the specific gravity of the liquor; the necessary corrections being applied by a scale or sliding-rule.
- The instrument here to be spoken of is constructed on this latter principle, which appears for many reasons to be the best: The hydrometer A B (pl. XV. fig. 3) is of brass. It is eight inches in length, with an elliptical bulb an inch and a half in diameter, and two inches long. The stem is square, each side being about $\frac{1}{2}$ of an inch wide, and, when the instrument is intended for such liquors only as are specifically lighter than water, is engraved only on one face with the 26 letters of the alphabet, and an O, or zero, at the top and bottom; opposite to each, and between every two of which, is a division for marking the point of the stem which is cut by the surface of any liquor in which it floats, the whole number of divisions being 55, as shewn in the plate. The weight of the instrument is about 400 grains, and it is provided with four weights, marked 1, 2, 3, 4, weighing respectively 20, 40, 61, and 84 grains, to be applied as occasion requires on the shank of the instrument C, on which they are retained by the button or fixed weight B. These weights are so adjusted, that when with any one of them, as for instance No. 2, the stem of the instrument, when floating in a given liquor, just emerges to the lower division O, it will, on changing the weight for the next heavier, No. 3, become immersed exactly to the other division O, towards its superior extremity. The stem is by this means virtually extended to five times its real length, and the number of divisions in effect augmented to 272. Thus without any weight at all, as represented in the figure, it would sink in a liquor whose specific gravity was .806, exactly to the upper division O,

O, and in one whose specific gravity was .843, to the lower division O; the intermediate divisions answering to intermediate specific quantities. With No. 1, it indicates specific gravities from .843 to .880; with No. 2, from .880 to .918; with No. 3, from .918 to .958; and with No. 4, from .958 to 1.000; the surface of water in which the instrument is made to float with the latter weight, exactly coinciding with the lower O, at 55° of temperature.

The whole interval from .806 to 1.000, comprehending the specific gravities of all spirituous compounds, is in this manner divided into five nearly equal portions, each of which, comprizing from .038 to .040, is measured by the whole length of the stem; so that each of the 54 intervals on its face corresponds to considerably less than an unit in the third place of the specific gravity, and indicates a difference of about one half per cent. or two quarts in 100 gallons, in the quality of the compound, and the instrument is therefore abundantly sensible to very minute variations in these respects.

With regard to the application of the weights, no error can possibly be committed. If the instrument floats in any liquor, so that its surface cuts any part of the stem, it is properly loaded. Any weight but the proper one will either sink it entirely, or suffer the stem to rise totally above the fluid.

The specific gravity indicated by any division on the stem of the hydrometer, is seen instantly by reference to a sliding-rule belonging to it, whose two faces are shewn, plate XV. fig. 2 and 3, on which the lines of divisions A A, A A, marked with the letters of the alphabet, represent those on the instrument when loaded with the weight, whose number is marked over the O at the commencement of each series, and the exterior lines of divisions B B, B B, near its edges, shew the corresponding specific gravities compared with that of water at 55°.

In order, however, to ascertain the strength and value of the spirit, it is necessary to examine its temperature and apply the proper correction, which is done with extreme accuracy by the same sliding-rule, by an ingenious application of two scales of unequal parts to each other, viz. the lines C C, C C, marked "Atkins," and the lines A A, A A, marked with the letters of the alphabet. The mode of doing this is as follows: the temperature of the liquor being taken by the thermometer accompanying the instrument, the asterisk or index, plate XV. fig. 1, by setting an index on the slider to the degree of temperature as shewn by the thermometer.

Division on slider answering to the letter on the stem gives per centage.

Example from the rule set as in the plate.

Uses of the other lines on the slider.

Of the concentration-line.

Concentration to be deducted from the per centage.

Example. Diminution of 3 parts on adding 50 of water to 100 of 50 over-proof.

Concentration on mixture of proof-spirit and water.

on that part of the slider opposite to the thermometrical scale D, is to be set to the degree of temperature so found, and the division on the line C, marked "*Atkins*," which then corresponds on either side of the rule with that letter and division on the alphabet line A, at which the stem was cut by the surface of the liquor, indicates the strength and value in commercial terms. Thus suppose the temperature to be 68° (at which the index appears set in the plate), and the hydrometer, when loaded with its weight No. 1, to float with the surface, cutting the division I on the stem (corresponding to a specific gravity of about $854\frac{1}{2}$), we find, on setting the slider of the rule to 68° on the thermometer scale, that 46 over proof corresponds to that letter and division, and is consequently the strength and value of the liquor.

The lines marked "*Dicas*" and "*Clark*" on the rule, are for giving the appreciation of the values and strengths as shewn by the instruments invented by those artists, and are intended for the use of such as have been in the habit of employing them; the latter being perhaps inserted principally on account of the use of Clark's instrument for the purposes of the revenue.

The figures on the line marked "*concentration*" amongst the *over-proofs*, indicate the diminution in volume which takes place on reducing the given compound to proof; and amongst the *under-proofs*, the diminution which takes place on reducing proof-spirit to that strength. This must in the first case be deducted from the quantity marked on Atkins's line (C), in order to obtain the accurate per centage according to the estimation of commercial men. For instance, 50 over-proof on Atkins's line indicates, that if to 100 parts of spirit of the strength which answers to this division, we add 50 of water, we shall get proof-spirit, of which, however, the quantity will be only 147 parts; the concentration or diminution of bulk by mixture being 3 parts. With regard to the spirit which is under-proof, the figures on the concentration line indicate the diminution of volume in pints for every 100 gallons of the mixture: if, for instance, 70 gallons of proof-spirit be combined with 30 of water, we get a compound whose strength is that which is marked on the line C, 30 under proof; but the concentration being seven pints, we shall get only $99\frac{1}{8}$ gallons.

When

When the hydrometer is intended only for spirituous com- pounds, the weights are applied singly as before-mentioned : if, however, in addition to the weight No. 4, the others are successively applied, it becomes applicable to the examination of worts and other liquors, whose specific gravities are from 1.000 to 1.109, or, in the language of the brewery, up to 40 lbs. per barrel heavier than water. In this case the other three sides of the stem are also graduated, and another rule with an ivory slider, carrying a thermometrical scale for comparing worts at different temperatures, is included in the case with the instrument.

Applicable to worts by applying the weights doubly.

Spec. gr. of worts designated by lbs. per barrel.

Additional rule for the brewery.

The writer of this paper having made a considerable number of experiments with this instrument, on the specific gravities of a variety of spirituous liquors, had originally intended to have given their results; but it has already run to such a length, that they must be deferred to a future communication. The errors, even including those which must necessarily arise from the various temperatures of the compounds, and the different quantities of foreign matters with which these fluids, in an impure state, may be supposed to be charged, appear to fall within very narrow limits; and the extreme facility and expedition with which it resolves the questions to the solution of which it is applicable, cannot fail to render it very highly valuable to those for whose purposes it is principally intended.

Errors in results very small, and advantage of facility of use very considerable.

XIV.

An Examination of Sig. VOLTA'S Experiments which he calls fundamental, and upon which his Theory of Galvanism rests; with a Description of a very sensible Electrical Condenser, and an Explanation of the Action of the Electric Fluid in the Galvanic Instrument. By JOHN CUTHBERTSON, Philosophical Instrument Maker, No, 54, Poland Street, London. Communicated by the Author.

IN Vol. I. 8vo, page 136, paragraph three, of this Journal, Sig. Volta affirms, "If two insulated discs, one of copper and the other zinc, be applied together for a moment and then separated, the zinc will be positive and copper negative." I have always had the same result, but some times much weaker than at others.

Volta's expt. of the contact of insulated discs of copper and zinc producing + el. in the zinc, and - el. in the copper. Succeeds.

Page

Expt. of copper and zinc joined, and the copper to touch a zinc condenser, and afford — el. in this last. Page 137, in the last paragraph, it is said, "If a piece of zinc to which is joined a piece of copper, and the zinc held by two fingers or in any other manner, and the copper made to touch the superior disc of the condenser, which is zinc, while the inferior is in communication with the ground, a moment afterwards raising the upper disc in the air, it will be negatively electrified." This I have always found the contrary, *i. e.* *positive*.

Expt. of zinc touching a copper condenser, and affording no el. Page 138, paragraph three, it is also said, "If the superior disc of the condenser be copper, and a piece of zinc be made to touch it immediately without any intervening substance, nothing will be obtained, because the zinc being then in contact at the two opposite ends with copper and copper, two equal forces act in opposite directions, and by that means destroy or counterbalance each other." If the superior disc was copper, I always found it negative, and if it was zinc touched by a piece of copper, it was positive; both contrary to Sig. Volta's assertion.

The foregoing expts. were often repeated.

We find then that only one experiment out of three succeeds, which he calls fundamental, upon which his theory of galvanism is founded.

Knowing Sig. Volta's abilities both as a philosopher and experimentalist, I own I mention the result of the foregoing experiments (though it is after numerous repetitions) with much diffidence; but the experiments are so simple, that it is almost impossible that I can have erred.

By reasoning upon the phenomena, when copper and zinc are made to touch each other, it becomes easily explicable by the old known laws of electricity.

EXPLANATION.

Explanation. In expt. 1. either the zinc attracted or the copper repelled the elect. while in contact.

In the first experiment, where zinc and copper are made to touch each other, we find, on separation, that the zinc has acquired a greater share of electric fluid than it had before the touch, and the copper less, by virtue of their mutual action upon each other when in contact. So that the zinc must have either attracted the electric fluid out of the copper, or the copper must have repelled it from itself into the zinc; and that the first is the true cause may be proved by connecting the copper disc with conductors, and then touching it by the insulated zinc, which will be found, on separation, to be much more strongly positive

positive than when the copper was insulated. For whatever quantity of electric fluid may be drawn out of the copper by the zinc, is recovered by its being in connection with continued conductors; whereas, if it had been repelled from the copper into the zinc, it must be stronger when insulated, because when insulated, the copper has no other body but the zinc to receive that which it repels from it; and when not insulated, every other conductor with which it is connected takes a part in proportion to their conducting property. As the zinc disc is found to be much stronger when the copper disc is not insulated, I conclude that zinc has the property of attracting the electric fluid out of copper when they are in contact.

In regard to the second experiment, where the zinc is found to be much more strongly positive than in the first, is clear from what I have already said, *i. e.* because the copper disc is not insulated, but is free to act as their mutual action upon each other may require.

In the third experiment, in which no effect should follow (according to Sig. Volta), the zinc was found to be as strongly electrified as in the last experiment. Neither the zinc nor the copper being completely insulated, they are at liberty to act upon each other as their mutual contact directs; the zinc at liberty to attract and the copper to give, which is in no wise repugnant to the old laws of electricity.

How Sig. Volta could be led to such erroneous conclusions is not easy to understand, unless he was deceived by the condensers he made use of. "To render that feeble electricity sensible and manifest, he recommends flat metallic plates covered with a slight layer of sealing wax or lac varnish," which I was not a little surprised at, because I have always found such condensers very equivocal in their results, and shew different signs without any variation or obvious cause, or at least such as I was unable to detect. They are more easily excited by a negative power than by a positive one, and retain it much more tenaciously; and hence, when they happen by any means to be strongly electrified by a negative power, it is almost impossible to discharge them of it, so as to be fit to proceed on immediately with experiments that require nice investigation.

Since the invention of the galvanic instrument, various electrometers, condensers, doublers, multipliers, &c. &c. have been used to investigate its electrical properties, all of which appear

The effect is stronger when the zinc alone is insulated, and less when the copper alone is insulated; hence the zinc is inferred to attract.

In expt. II. the insulation of the zinc increases the effect.

Expt. III. The metals being at liberty to assume the respective states on contact, did so accordingly.

Sig. Volta supposed to have been deceived by his varnished condensers.

Mr. John Read's construction of the condenser.

appear to me to be much inferior to one constructed in the year 1796, by Mr. John Read, an ingenious electrician at Knightsbridge; but as he invented it near the time of his retiring from business, he did not publish it, consequently it is known but by very few electricians. I have found it of great use not only in galvanism, but in all experiments wherein small quantities of electricity are to be made evident; and I doubt not but that it will be considered as a valuable acquisition by electricians. I have found it capable of rendering much smaller quantities sensible than any other instrument. By this instrument we are able to learn the positive and negative sides of only one piece of zinc, copper, and wet cloth; and I have not heard that it has been done by any other instrument less than with a series of twenty*.

A Description and Use of Mr. Read's compound Electrical Condenser.

Description of
Read's con-
denser.

Fig. 1. plate XVI. represents a vertical section of the large condenser: *aa* is a round flat plate of brass of about eight inches, standing insulated upon a wooden foot *g*; *gh* is a hollow brass cylinder, and *f'h* is a solid stick of glass, with a brass mounting at *f*, to which the plate *aa* is fixed; *bb* is another plate of brass somewhat less in diameter, and has a round hole in the middle of about two inches diameter; *cd* is a hollow cone fixed to *bb* at *cc*; *de* is a hollow brass cylinder fixed to the cone at *d*, and made to slide up and down upon the cylinder *gh*; *i* is a milled head-screw which holds *bb* fast when it is at its proper distance from *aa*, where a stop is made for that purpose; when *i* is loose, *bb* falls down by its own weight, and rests upon the foot *g*.

Altered by the
author.

The above is the original construction, which I found more complex and less portable than I could wish; for which reasons I make them in the following manner; and fixing the condensing plates in a vertical position instead of horizontal, it has all the advantages, is more simple, and perfectly portable, and I have no doubt will meet with the approbation of Mr. Read himself.

Description of the improved Condenser.

Description of
the improved
condenser.

Fig. 2 represents a vertical section of the large condenser standing edgewise to the eye: *aa* and *bb* are two flat round

* Bennet's electrometer alone shewed the state of 40 pairs. See Van Marum in Philos. Journal, octavo, I. 174.—N.

brass.

brass plates of about six inches; *c* is a glass cylinder fixed at one end into a wooden foot *d*; *e* is a brass ball, and mounting at the other end to which the plate *bb* is screwed fast; *f* is a brass wire with a joint at its lower end, and at the other end a ball to which the plate *a a* is screwed, standing parallel to *b b*. By means of the joint, the plate *a a* may be moved back in the situation of the dotted outline *a g*. The joint has a shoulder which stops the plate *a a*, and keeps it at its proper distance from *b b*.

Fig. 3 is the common gold-leaf electrometer, with an addition of two vertical brass plates of about $1\frac{1}{2}$ inch in diameter; one is screwed fast to the brass mounting at the top of the electrometer, and the other to a wire which has a joint fixed to the foot of the electrometer, by which it is moveable, and is set either parallel and at a proper distance from the other plate, the joint being furnished with a stop for that purpose, or may be moved backwards in the direction of *ga*, fig. 2: *l* is a brass cup with a shank at the bottom, which screws into a hole in the top of *e*, fig. 2, serving to examine the state of electricity excited by dropping of metals, &c.; *m* is a stick covered with tinfoil, which screws into the hole at *e*, to examine the electricity of the atmosphere; *n* is a brass wire jointed at *o*, with a shank, which can also be screwed into *e* when required, or into the top of the gold leaf electrometer, fig. 3. These two instruments, fig. 2 and 3, are used separately or combined, as the nature of the experiment may require. When the experiment requires the aid of both condensers, they are combined, as is represented fig. 4, the two fixed plates standing towards the eye. The fixed plate of the large condenser has a small brass pin at one side, which, when the instruments are used together, must touch the fixed condensing plate of the gold-leaf electrometer *.

Bennet's electrometer with a condenser.

THE METHOD OF USING THE DOUBLE ELECTRICAL CONDENSER.

To shew the Electric Fluid excited by Effervescence, &c.

Screw *l* into the top of the ball *e* of the large condenser, and set therein a china or glass cup with proper ingredients for that

Use of the two condensers.

To shew the electricity from effervescence, &c.

* Instead of gold leaf Mr. Read uses very fine fibres of flax, which he thinks more sensible than gold leaf; but they are very difficult to be seen, and more easily deranged. If gold leaf be properly managed, I think it preferable.—C.

purpose;

purpose; then join the two instruments as in fig. 4. While the effervescence is going on, turn back the moveable plate of the large condenser into the position of the dotted line, fig. 2, taking care not to touch the fixed plate; then if the excited electricity be very strong, the gold leaf will diverge; but if not, just move the electrometer so that it is quite free from the pin, turn back its moveable plate, and if a sufficient quantity of electric fluid be excited, the gold leaf will diverge.

To shew if there be any sensible Quantity of Electric Fluid in the Atmosphere.

Atmospheric
electricity.

Screw the stick *m* into *e* of the large condenser; a convenient place being chosen not much surrounded by buildings or trees, set the instruments combined as in the last experiment, and proceed in the same manner.

Method of applying the combined Condensers to the Galvanic Instrument.

Method of ap-
plying the con-
denser to the gal-
vanic instru-
ment.

Screw the short end of *no* into *e* of the large condensing plate (the instruments being combined as in the former experiments); bend the end downwards, at such a distance from the table, or whatever it may stand upon, that the two pieces of metal, zinc and copper, as at *n*, can be put under it, and drawn away from under it again, without its touching the table when the metal is drawn away. Take two pieces of metal, zinc and copper, about the size of half a crown or upwards, either separate or foldered together, with their flat sides in contact, and push them under the end *n* of *on*. After remaining a short space of time, a quarter or half a minute, draw them away from under the point, and take notice that the point does not touch the table, or any other conductor; then turn back the moveable plate of the large condenser; move the electrometer so that its plate shall no longer touch the pin of the large plate, and then turn its moveable plate back; the gold leaf will remain undisturbed.

Electricity from
the galvanism of
a single pair of
plates.

Turn up the condensing plates to their first position; place the two instruments together as before, taking particular care that the fixed plate of the electrometer condenser touches the pin proceeding from the large plate. Lay upon the pieces of metal before used, a piece of woollen cloth well soaked in a solution of muriate of ammonia, or any other menstruum com-
monly

monly used for the galvanic experiments either upon the zinc or the copper, and push them under the point of *n* again. Press the point down upon them, that it may be perfectly in contact; after they have remained the time before-mentioned, draw the metals away, and separate or turn back the large condenser plate, and also the small one, after separating it from the pin of the large one, and immediately the gold leaf will diverge. If the zinc was the uppermost, then the gold leaf will diverge with positive electricity; but if it was underneath, the gold leaf will diverge with negative electricity. It makes no difference in the general effect, upon which metal the wet cloth was laid; or whether two pieces of cloth were used, one under the metals and the other above; or only one either above the metals or under them. But if the cloth be only laid upon the copper and not upon the zinc, the electric fluid brought into action will be so weak, that the combined instrument can hardly shew it; if laid upon zinc, the divergency will be about $\frac{1}{40}$ of an inch; sometimes more and sometimes less*.

By reflecting on this phenomenon I found the following explanation, without having recourse to any new hypothesis.

Explanation of the Action of the Electric Fluid in the Galvanic

When flat pieces of zinc and copper are laid in contact, the zinc becomes positive and the copper negative at the moment of the touch; and while they remain in contact, the electric fluid contained in them both is perfectly in equilibrio. The copper has given and the zinc has received such a quantity of electric fluid as their mutual action upon each other required; and in consequence of this property, they present a mutual resistance to any further change being produced upon them. If then any menstruum be added to the opposite side of the metals, capable of producing a change in their metallic property (such as the fluid contained in the wet cloth), a change in their electrical property must of course follow. But as this change in the metallic property is only superficial, it will only be there that its electric property is changed. The other parts of the two metals in contact will remain unaltered, and maintain their property of resistance. The change produced by the action of

Explanation of the facts offered. Zinc and copper in contact arrive at an equilibrium of electricity; the zinc + and the copper —.

Chemical action alters the electrical property of that part of the metal in which it takes place.

* When the atmosphere is in a favourable state. In this as well as all other experiments where so small quantities are to be made evident, the atmosphere has great influence.

This change is asserted to be of an opposite nature to the electric state of the unaltered metal, and therefore produces electric states at the wet surfaces opposite to those of the dry metals: which do not pass from zinc to copper *through the metals* and become quiescent; but from zinc to copper *through the fluid*, this passage being easiest. Hence the galvanic current: which is progressive, because the fluid cannot perfectly conduct the electricity as fast as it is extricated.

The shock from the accumulation suddenly transmitted.

the menstruum, with respect to its electrical property upon that part only of the metal whereon it has acted, is exactly the reverse of that of the parts not acted upon. The part of the zinc thus acted upon, must consequently be disposed to throw off its electric fluid, and would give to that part of the copper, which by a like action is disposed to absorb it) so that the two states of the surfaces acted upon would unite and counterbalance each other imperceptibly) if it were not opposed by the still existing property and mutual action of the two metals in their parts not acted upon by the menstruum; hence it follows that the electric fluid is propelled forwards from the zinc *through the menstruum* to the next adjoining copper in the pile or trough; but this can only happen in a progressive manner, on account of the menstruum being an imperfect conductor, which appears to be an indispensable condition to the maintaining any electric intensity.

The shock or sensation felt on touching the two opposite ends of the galvanic instrument, depends upon the menstruum, together with the resisting property of the two metals in contact. The fluid must be adequately proportioned between being an electric and a perfect conductor. If it were a perfect conductor, the electric fluid would pass from the zinc through the menstruum to the next adjoining copper, as quick as it is given off by the altered part of the zinc; no accumulation would ensue, and consequently no sensation of shock or discharge would be perceived. If it were an electric, the electric fluid given off by the altered zinc would be stopped, and accumulate till it become possessed of sufficient force to overcome the mutual resistance of the two metals in contact, and pass through by a reverse motion to the copper; consequently no sensation would be felt by forming a communication between the two opposite ends. For though there would be an accumulation, yet it cannot be united so as to act in concert with that of the other combined metals; being shut off by the interposed electric menstruum, and too feeble of itself to affect our senses.

ANNOTATION. W. N.

Short history of instruments for shewing minute quantities of electricity.
W. N.

At page 396, vol. I. of the quarto series of this Journal, an outline is given of the history of all the instruments for shewing or measuring minute quantities of electricity, with a statement of their advantages and defects. With regard to the condenser discovered

discovered by Volta in 1780 *, and used for atmospheric electricity and in the grand discovery of the electricity produced by chemical changes, it was applied very early by Cavallo in the compound form; that is to say, by charging a smaller instrument with the contents of a larger. Mr. Bennet before 1787 †, added the condenser to his electrometer, and invented the method called 'doubbling, which had before been used with the electrophore by Lichtenberg ‡ and Klinckow ․. Our electricians, particularly Mr. Cavallo and Mr. Bennet, were at this time fully aware of the spontaneous electricity of the doubler, and the evils to be apprehended from the varnish and the actual contacts. I think it was Mr. Cavallo who substituted small knobs of sealing wax instead of varnish, which I saw adopted in a mechanical doubler by Dr. Darwin in 1787; and in the year 1788 I communicated to the Royal Society || the revolving doubler, in which the plates approach and recede without touching; and soon afterwards I made the spinning instrument, consisting of a condenser and an electrical well. In the same year Mr. Cavallo published ** his *collector* of electricity, which is a condenser having two uninsulated plates, which separate like plate *a*, fig. 2, on each side of *b*, without contact; and in the third vol. of his "Complete Treatise on " Electricity," 1795, he describes his *multiplier*, which consists of two condensers, equally perfect; one of which is made to charge the other by repeated alternations of the compensating plate.

The above short sketch will enable the reader to ascertain to what extent the ingenious contrivers of the instruments in plate XV. have availed themselves of the labours of former operators.

* Journal de Physique for May and August 1783, and Phil. Transf. LXXII. p. 237.

† Phil. Transf. LXXII. p. 32.

‡ Journal de Phys. Jan. 1780, p. 20.

§ Phil. Transf. LXVIII. p. 1029.

|| Phil. Transf. LXXVIII.

** Phil. Transf. LXXVIII.

XV.

Observations on the Phosphorescence of the Tremolite, and of the calcareous Phosphate of slow Solution, known by the Name of Dolomie. By M. LE COMTE DE BOURNON, Fellow of the Royal and Linnæan Societies. Translated from the Original; communicated by the Author.

The phosphorescence of minerals has been little inquired into.

The methods of exciting it: 1. by friction; 2. by heat.

Does it arise from combined light?

FEW researches have yet been made concerning the phosphorescence of the bodies of the mineral kingdom. No satisfactory explanation has yet been given of this phenomenon, though this knowledge would undoubtedly throw new light upon the study of minerals, and prove a great acquisition to natural philosophy and chemistry.

This property of emitting light, which daily observation shews to belong to many more minerals than had formerly been suspected, requires particular management. In some fossils, such as quartz, blende, corundum, &c. &c. it becomes sensible only by friction. In others it exhibits itself only when the mineral is placed upon a red hot coal, or upon any other body heated to a similar temperature. This is the case with the carbonate of strontian, of barytes, &c. In others again, the phosphorescence is developed both by friction and by heat, as is the case with the phosphates and fluates of lime, as well as with a great number of carbonates of the same earth, particularly those of a brown or yellowish colour.

These facts give occasion for several questions, the solution of which would be extremely interesting.

Do these two kinds of phosphorescence depend upon the same cause? In all the stones which exhibit them, and are at the same time coloured, the colour diminishes in proportion to the disengagement of the phosphorescence by the action of heat: and when they cease to be phosphorescent, they at the same time intirely cease to be coloured. Does this phenomenon proceed from the disengagement of the combined or interposed light? Does the colour in these stones always belong, in reality, to metallic oxides, particularly those of iron? May it not rather belong simply to the combined light? In this case, may it not be supposed that the light is decomposed, by combining with these stones, and that it then entered into their composition

composition only by insulated rays, or by combination of two or more, and not the whole of the rays? We are, however, enabled to make an observation with respect to this subject, namely, that these stones constantly exhibit by the action of heat, a phosphorescence of the same colour, whatever may be the colour of their proper substance. For instance, the fluates of lime which exhibit the most lively and variegated colours, constantly give a light inclining to the violet, with the single exception of the Siberian variety, which has been named the Chlorophane, and which, though of a violet colour, gives a phosphorescence of a beautiful emerald green. In others, as in some carbonates of lime, in those of barytes, of strontian, &c. though these stones are perfectly colourless, the phosphorescence is constantly reddish, or orange yellow. What may be the cause of these contrasts?

In some cases the cause which produces the phosphorescence of the stones seems to belong to an essential part of their substance, which is never completely expelled from them. This is the case with the calcareous fluates and phosphates, &c. In others it appears to be purely accidental in the stone, and shews itself only in a certain number of individuals belonging to the same species. In the first case, this property ought to be indicated amongst the specific characters of the stone; in the second, it cannot serve as character of the species, but can merely be used to designate one of its varieties. Such is, for example, that which exists in the tremolite and in the dolomie, respecting which I intend here to offer some observations, which appear to me to deserve the attention of mineralogists.

M. de Saussure and Professor Blumenbach were, as far as I know, the first who observed the two kinds of phosphorescence in the tremolite; and, since them, all the works of mineralogy have placed this property amongst the distinctive characters of this stone. Many tremolites, indeed, are endowed with this double phosphorescence; but this is by no means the case with all, nor can this character be considered as essential to its nature.

The tremolite, both that which is found in different vallies of Mount St. Gothard, and that which is brought to us from a great number of other places, is generally inclosed in a granulated carbonate of lime, the grains of which are of various degrees of fineness, and their adhesion more or less considerable.

The phosphoric colour is the same in all stones of the same species, however themselves may be coloured.

It may arise from a permanent cause, or from some portion that can be abstracted.

The tremolite is not in all specimens phosphorescent.

Those which are in a phosphorescent gangue are themselves phosphorescent; and on the contrary when the gangue is not phospho-

rescent the tremolite is not so.

Among these carbonates of lime, which constitute the gangue of the tremolite, and very frequently belong to the species which is called dolomie, a great number are endowed with the double phosphorescence; but, on the other hand, we meet with several, in which not the slightest trace of this property can be found. The tremolites inclosed in the first partake, though in rather an inferior degree, of their phosphorescence, whereas those which are inclosed in a non-phosphorescent carbonate of lime are equally void of that property.

That the phosphorescence arose from interposed carbonate of lime, was proved by taking it up by nitric acid, which destroyed the luminous quality.

From the very first moment when I observed this fact, it occurred to me, that the phosphorescence of the tremolite might very probably proceed merely from that of the carbonate of lime, which undoubtedly is interposed between its parts. I therefore selected some crystals that were inclosed in phosphorescent carbonate of lime, and after having satisfied myself that they were themselves phosphorescent, both by friction and by the action of heat, I digested them for some time in nitric acid. When I took them out, their surface was perforated with small cavities, occasioned by the solution of the portions of carbonate of lime, and friction was then incapable of making them display the slightest phosphorescent light. This light, however, was emitted, at the point immediately after ignition, though in an extremely weakened degree. I afterwards reduced some of the same crystals to a coarse powder, and this powder having remained again for some time in the acid, was intirely deprived of all its phosphorescent property.

This quality therefore belongs to the variety, and not the species.

I could no longer doubt that the carbonate of lime, interposed between the particles of the tremolite was the real cause of the phosphorescence of this substance, when its gangue was endowed with the same property. It therefore appeared to me to be at the same time perfectly proved, that phosphorescence cannot be ranked amongst the characters of the species in this substance, and that it ought to be considered merely as distinctive of one of its varieties.

Whether the large portion of lime said to exist in tremolite can be admitted as a constituent part?

A new doubt, which was a very natural consequence of this observation, now presented itself. Could it be true, that the lime which chemists have reckoned to amount to $\frac{1}{100}$ amongst the constituent parts of the tremolite, does exist in it in so large a proportion. In order, if possible, to satisfy this doubt, I selected some crystals of non-phosphorescent tremolite, which

had only an argillo-martial substance for their gangue, and requested Mr. Chenevix, who has already rendered such useful services to mineralogy, to examine them by analysis. I likewise gave him some crystals from amongst those phosphorescent ones which I had broken, and afterwards deprived of their phosphorescence, by digesting them for some time in nitric acid.

My suspicions were verified: Mr. Chenevix found only $\frac{4}{100}$ of lime in each of these two analyses. But what at the same time struck me, was that the tremolite taken from the phosphorescent variety, having for its gangue a carbonate of lime of the species called the dolomie, which is likewise phosphorescent, but whose calcareous part had been taken away by the nitric acid, gave by analysis only $\frac{4}{100}$ of argil, whilst that which was taken from an absolutely argillaceous gangue gave $\frac{1}{100}$ of the same earth. Mr. Klaproth having found no argil at all in the analysis which he had before made of this substance, it is probable that its existence in it, like that of the calcareous carbonate, proceeds merely from its simple interposition.

These two observations appeared to me to be very interesting to the study of mineralogy, especially to that part of it which relates to analysis; since it shews with what care the chemist ought to avoid confounding, with the true constituent parts of a substance, those which are foreign to it, and only interposed between its parts. It is extremely common to find the parts of a mineral, even in the state of crystallization, envelope more or less of the portions of that substance which constitutes its gangue; and what may likewise contribute much to mistakes in this respect, is the kind of constancy with which, (whatever may be its cause, has hitherto been little attended to) the same substance, placed in similar circumstances, admits this interposed extraneous substance, in equal, or nearly equal proportions. It is not therefore sufficient that the chemist should select for his analysis, amongst the crystals of a substance, those which appear to him to be the most pure, (and the perfection of their form and transparency is the strongest presumption which he can have in this respect;) but he should also repeat the same analytical process upon the same substance taken from totally different gangues.

Probability that a portion of the gangue, whether lime or argil, is interposed in the mineral.

Hence it is of importance that chemical analyses of any mineral should be repeated upon specimens taken from different gangues.

When

The phosphorescence of tremolite being admitted to arise from interperfed carbonate of lime, will account for the differing observations of authors.

Though the phosphorescence of tremolite cannot be admitted as specific, yet the facility with which this very hard mineral is crushed by pressure with the hammer and its elastic resistance, are peculiar specific characters.

Tremolite is found in Scotland, at Mount Vesuvius, and in Bengal.

Description of the tremolite of Scotland.

When it is once admitted that the tremolite is phosphorescent only in proportion as this property is contained in the carbonate of lime interposed in its substance, the variations which some authors have found in its phosphorescence, will be easily accounted for. It must, for example, be the more easily obtained by friction, as the hardness of this stone is less considerable; because the friction, by breaking its surface, will successively arrive at the interposed particles of carbonate of lime. And hence it is quite natural that the fibrous varieties should be more phosphorescent than the others, and that these should be the less so in proportion as they are harder.

As this character, which was represented as specific in the tremolite, must no longer be considered as such, there is one which has been overlooked, and which, in my opinion, ought to be added to those which have been already observed in it; I mean the great facility with which, notwithstanding its hardness, which, in the purest specimens is such that it easily cuts glass, it is crushed by the mere pressure of the hammer, and the kind of flexibility which it then exhibits. If, in breaking it, the pressure be moderate, the crystals of tremolite divide, pretty generally, according to the length of their prisms, into small fibres, which are frequently as fine as those of the amianthus, to which, in this state, they have much resemblance. We may then even increase the pressure without breaking the fibres, which in this case afford by their resistance the same sensation as is felt from a slightly elastic body. This effect, as well as the reduction of the tremolites into small fibres, is more distinct the less pure the mineral. Both properties however are observed, but in a much less degree in the purest tremolites, and consequently in those whose hardness is the most considerable.

With a view to add as much as possible to the knowledge already acquired respecting this substance, I shall add to the list of places which have been indicated as the native countries of the tremolite, Scotland, Mount Vesuvius, and Bengal. Mr. Greville's rich cabinet in London contains specimens from each of those different places; a description of these will probably be acceptable to mineralogists.

The tremolite of Scotland is in the fibrous state, its fibres being very fine and close, part of which is disposed in divergent

gent rays, transversely crossed by other fibres, so as to represent a kind of texture, as is sometimes the case with the zeolite mezoïpe. This tremolite is of a greenish white colour: it adheres to a granulated but very compact mass of carbonate of lime, mixed in almost equal proportions with the same tremolite, also in a granulated state, on which account it strikes fire with steel. This carbonate of lime possesses both kinds of phosphorescence, but the light which it gives is of a slightly blueish colour; the same is the case with the tremolite which it contains. This carbonate does not belong to the dolomie, and it dissolves in acids in the same manner as the common carbonate of lime.

The tremolite of Vesuvius is likewise in the fibrous state, with fine and close fibres; its colour is a greyish white, it adheres to a gangue composed of carbonate of lime, of an immense quantity of very fine small fibres of the same tremolite, and of a great number of very small crystals of pyroxene, of a beautiful green colour and transparent. Some portions of idocrase are also observable, of which there is a group at one of the extremities of the specimen in pretty large crystals. The carbonate of lime does not belong to that of slow solution; it possesses both kinds of phosphorescence, and the light which it emits is of a very lively deep orange colour. The tremolite exhibits absolutely the same phosphorescence.

We owe our knowledge of the tremolite of Bengal to Sir John Murray. It is in pretty large crystals of a greenish grey colour, bedded separately in a granulated carbonate of lime, the very fine grains of which have a strong adhesion to each other; a character which, joined to the great whiteness of this stone, causes it very much to resemble a piece of double refined sugar. This carbonate of lime belongs to the species of the dolomie; it is even one of those in which I have observed the solution to be the slowest and most insensible, but it is nevertheless completely dissolved in the nitric acid, leaving only a light whitish and clouded residue, which disappears when the acid is diluted with water. Its hardness, which is much superior to that of the ordinary carbonate of lime, is rather inferior to that of the fluat of the same earth; and this is the case with all the dolomies, not excepting those whose grains have the least adhesion with each other. This dolomie is not
phospho-

Tremolite of
Mount Vesu-
vius.

Tremolite of
Bengal.

phosphorescent either by friction or by the action of heat, and the terebinth which it contains is intirely in the same state. There are many other delomies also which possess no phosphorescence, as has been also observed by the Abbé Hany. This character therefore still belongs to the variety, and not to the species.

XVI.

Outline of the History of Galvanism: with a Theory of the Action of the Galvanic Apparatus. By JOHN BOSTOCK, M. D. From the Author.

To Mr. NICHOLSON.

SIR,

Introductory
letter.

HAVING been lately employed in some experimental inquiries on the subject of galvanism, I found it commodious to arrange the numerous discoveries that have been made in this department of science into the historical form. The facts which have been successively developed begin to assume so elevated a rank among the branches of natural philosophy, that a sketch of the most important and best established amongst them seems desirable, in order that the experimenter may be easily enabled to see what has been done by his predecessors, and may thus be prevented from wasting his time and exertions upon points which have been previously investigated. From reflecting upon the labours of others, and comparing them with my own experiments, I have been led to form a theory of the action of the galvanic pile, which seems to explain in an easy manner most of its singular properties. I am indeed well aware of the undue attachment which every one feels for the offspring of his own imagination, and I shall not be surprized if you perceive in the hypothesis many blemishes which have escaped my notice. I have however sent you both the history and the theory, in order that you may insert them in your Journal, if you think them deserving of a place there.

I am, SIR,

Your obedient Servant,

JOHN BOSTOCK, M. D.

Liverpool, June 1, 1802.

HISTORIC

HISTORIC SKETCH OF GALVANISM.

THE first publication * upon the subject is the work of Galvani himself, which appeared in 1791. It begins by giving an account of the following accidental discovery. A frog had its hinder legs separated from its body, except that the crural nerves were left undivided, and was by chance laid upon a table on which stood an electrical machine. It was observed that when the animal was placed in contact with, or sufficiently near to, an extensive surface of a conductor of the electric fluid, if a spark were taken from the machine in any direction, the legs of the animal were spasmodically contracted. When the frog formed part of the electric circle, so that the fluid passed immediately through it, a quantity almost imperceptible was found to excite contractions of the muscles. A much smaller quantity of the electric fluid produced the effect, when the animal was prepared in the manner described above, than when the body was left entire, because in the former case the fluid was confined to the narrow track of the nerves in its passage along the circuit. A *prepared frog* appears therefore to be a most delicate electrometer, as it exhibited contractions where no marks of electricity could be discovered by the instruments either of Bennet or Cavallo. Galvani afterwards found that contractions could be produced in the limbs of prepared frogs by the electricity of the atmosphere, and it was in consequence of some arrangements which he made for this purpose, that he was led to his great discovery of animal electricity. He found that he was able to produce contractions in the limbs of frogs without the aid of any foreign or artificial excitement, merely by the application of a conducting substance from the nerve to the muscle. These contractions were capable of being produced, of whatever substances the circuit of communication was composed, provided only they were all conductors of the electric fluid. The effects were found to be much increased by applying a metallic coating to the nerve. He found this peculiar species of electricity to exist in a great variety of animals, and that the contractions may be excited

First publication on galvanism by Galvani in 1791. Legs of a frog, separated from the body, were convulsed by extremely minute portions of electricity.

Proper galvanism, or contractions without excited electricity.

* Sultzer in his *Theorie des Plaisirs*, quoted by Fabbroni, *Philos. Journal*, quarto IV. 120, mentioned the taste by contact of two metals, -- N.

either

Theory of Galvani; that the interior of the muscle is charged plus, and the nerves form a communication with the outside.

*Valli's Letters on Animal Electricity, 1793.
Volta's Letters to Cavallo, 1793.*

No charge in the animal.

but electricity from the contact of different metals applied to the nerves.

The galv. action supposed to exist in nerves only.

either in the whole body or in particular parts of it, as long as the animal possesses any remains of vitality. Galvani supposed these phenomena to be analogous to the effects of the Leyden phial; that there is an excess of electric fluid in the interior of the muscle or in the nerve, and a deficiency on the outside, or *v. v.* The nerve he conceived to act the part of the wire in the Leyden phial. Soon after the publication of Galvani's work, Valli's Letters on Animal Electricity appeared in the *Journal de Physique*, vol. 41. & seq.

In the Transactions of the Royal Society for 1796, Volta's letter to Cavallo was published, which besides giving an account of Galvani's discoveries, contains many original experiments and observations. The analogy of the Leyden phial he shows is without foundation, for he found that he could excite similar contractions in the limbs, when the conducting circuit only touched two parts of the nerve, two muscles, or two parts of the same muscle; in order to accomplish this, it is however necessary to use two different metals. He supposes that in these cases the muscular contractions are produced by a small quantity of electricity which is excited by the action of the metals upon each other; this he conceives to depend upon a general law of the electric fluid, and that its effects are visible in the experiments of Galvani only because the prepared animal is the most delicate species of electrometer. Volta endeavours to prove by experiment, that the action is always in the first instance upon the nerves, and that the muscles are only affected through their medium. He imagines that it is not necessary that a communication should exist between the nerves and the muscles according to the opinion of Galvani, he imagines that the contractions will be produced in the limbs, if the influence be only made to pass from one part of a nerve to another part of the same. But we shall find that in this idea Volta is probably mistaken, as in his experiments the moisture adhering to the nerve formed a communication between it and the muscle. He found that if different parts of a nerve, or indeed if the body of the animal in general, be laid upon two different metals, and these metals be made to communicate by a conducting substance, muscular contractions are produced. These experiments succeed with more certainty when the skin is removed; this precaution is more especially requisite if the animal have a dry skin, as is the case with birds and quadrupeds.

ped. In a second letter which is printed in the same vol. of the Phil. Transf. Volta pursues his experiments and observations upon the subject of galvanism. If a single muscle, or a part of it be armed with coatings of two different metals, and these be made to communicate by a conductor, the contractions will be excited, but no effect will be produced if two coatings of the same metal be employed. Worms and snails he found could not be excited by this influence; but flies, beetles, grasshoppers, and butterflies, he found were subject to its action. Upon the whole it appears that those animals only which have distinct limbs, with flexor and extensor muscles, are excitable by animal electricity. In those animals which are acted upon by the galvanic influence, Volta found that it was only the muscles which are under the direction of the will that can be made to retract; in his experiments he never perceived that the heart was affected by the two metals, though this organ is thrown into strong contractions by the slightest chemical or mechanical stimulus. The two metals which were found by their union to perform the most powerful effects were zinc and silver. Volta placed these metals one on each surface of the end of the tongue; when they were brought into contact no motion was produced, but a strong sensation of taste was excited. When the metals were applied to the root of a tongue cut from the mouth, contractions were produced.

Of the various animals, those only were found to be affected which have distinct limbs, &c. and those muscles on'y which are subject to the will.

Zinc and silver.

In the same year in which Volta's letters appeared in the Phil. Transactions, Fowler published an essay on Animal Electricity; these works must therefore be considered as equally original. The extracts given above from Volta's letters, prove that he considered the phenomena of galvanism to depend upon the operations of the electric fluid; other experimenters however conceived it impossible to reconcile the new discoveries with their previous ideas of the nature of electricity, and Fowler commences his treatise by this enquiry. In order to ascertain this point he investigates the circumstances which are necessary to the production of the muscular contractions. These he found to be the contact of the two metals with each other, and their communication with the animal; the contractions may also be produced by bringing the metals in contact with each other in water, without either of the metals touching the animal. When the metals are applied to

Fowler's Essay, 1793, Whether galvanism be electricity.

Conditions of the effect: that two different metals should touch each other and the animal; viz. the nerve the and muscle;

either immediately,
or mediately by
galvanism.

Attempt to dis-
prove the theory
of an electric
charge in the
animal system;
and to show that
galvanism is not
electricity.

Torpedo.

Effect on worms,
and on the in-
voluntary mus-
cles.

Discovery of the
flash of light.

Professor Robi-
son makes a
pile of zinc and
silver.

Darwin in 1794
considers galva-
nism as electri-
city.

the nerve alone, Fowler still considers it requisite that there be a connection between the nerve and the muscle; in ordinary cases this connection is effected by the moisture which adheres to the nerve, a circumstance which Volta seems to have overlooked. Valli had endeavoured to form a theory of animal electricity founded upon the idea that the electric fluid was unequally dispersed through the body, and that the application of the metals produced an equilibrium; Fowler performed a number of experiments to disprove this idea, and apparently with success. He concludes the first part of his essay by giving it as his opinion, that the phenomena of galvanism are not reconcilable to the known laws of electricity, because for the excitement of the electric fluid, motion between an electric and a conductor appears to be necessary, whereas in these experiments two metals only are the substances employed. He also failed in causing charcoal to conduct this influence, though it is a better conductor of electricity than the animal fluids; in this particular, however, he appears to have been mistaken. He considers the influence as very similar in its effects to the action of the torpedo, though not altogether the same. He was unable to produce the muscular contractions in worms, yet he found that when a worm or a leech lying upon silver, put its mouth upon zinc, it appeared to suffer great uneasiness. Dr. Fowler in the second place proceeds to enquire, whether all the muscles of the body be subject to this new influence. He found it difficult to excite any contractions in the heart, though at length, by using proper precautions, he succeeded; but he was not able to excite contractions in the stomach or intestines. He discovered that when the nerves of vision were acted upon by the two metals, a sensation of a flash of light was perceived in the eye. When inflammation was excited in a limb, it appeared to acquire additional sensibility to the galvanic influence. Fowler's work concludes with a letter from Professor Robison, who made some new observations upon the production of the galvanic flash, and first noticed the effect produced by applying the tongue to a number of pieces of silver and zinc alternately piled upon each other.

In 1794 the first volume of the *Zoonomia* was published; Darwin speaks of the phenomena of galvanism, and considers them as electrical. The muscular contractions he supposes depend

depend upon the sensibility of our nerves to small quantities of the electric fluid. Bennet discovered by means of his electrometer, that zinc, when separate from other metals, always is in the minus state, and silver in the plus state; when therefore they are brought nearly together, a small plate of air is charged like a Leyden phial; when the metals are brought into contact this is discharged.

Bennet's early discovery of the plus state of zinc (1789.)

In the Phil. Transf. for the year 1795, is a Paper by Dr. Wells, on the subject of galvanism. He proposes for consideration the three following enquiries; Do the contractions observed by Galvani depend upon any property peculiar to the living body? What are the conditions necessary for the excitement of the influence? Is it electrical? With respect to the first question, he supposes that animals act only on account of their moisture. With respect to the second, he found that one metal and charcoal excited the contractions as readily as two metals; in this he corrects the mistake of Fowler noticed above. He however found that all charcoal will not act as a conductor of the influence, in which opinion Volta agrees with him. Wells does not agree in the hypothesis adopted by Volta and others, that the contact of the metals produces an alteration in the disposition of the electric fluid, for he very properly asks, why should not the natural moisture of the animal afford a communication between the two metals before they are connected by any other conductor? He farther discovered, that contractions could be excited by one metal only when it had been rubbed upon another metal, or even upon the hand. Charcoal may by the same means be made to produce the same effect; he proved by a variety of experiments that the friction does not in these cases communicate electricity to the metal or the charcoal; we may conjecture that an incipient oxidation, or some other chemical change was produced upon the surface of these substances. With respect to the third question, Wells is decidedly of opinion, that the phenomena are electrical; the influence being conducted by all conductors of electricity, and by them alone.

1795, stated from experiments that animals are affected only by means of humidity; that the state of the metals is not altered by contact; but that the phenomena are electrical.

Besides the works here mentioned a number of communications appeared in the different scientific journals of Italy, France, and Germany, and several distinct treatises were published on the subject of animal electricity. Humbolt particularly

Various other papers, Humboldt, Dr. Monro, the Institute of France, &c.

Fabroni: that galvanism is strictly a chemical phenomenon.

History of this power in Cavallo's Electricity, and the Supplement to the Encyclopædia Britannica.

Grand discovery of the pile by Volta.

Description.

Galvanic shock.

ticularly distinguished himself by his assiduity in varying the experiments. Dr. Monro wrote upon the subject, and the National Institute of France published an elaborate report, drawn up by a Committee composed of several of its most learned members. As however these works do not appear to contain any facts which materially illustrate the nature of the galvanic influence, or lead us to form any more accurate notions respecting its operations, we shall in this brief sketch only notice the papers of Fabroni, of which an account may be found in the 4th Vol. of Nicholson's Journal. He deduces from his experiments that galvanism is intirely a chemical phenomenon; he finds that metals become oxidated when in contact with each other in circumstances where this would not take place if they were kept separate. This idea he confirms by many observations and experiments, and supposes that when the galvanic influence is excited by the action of two metals that a chemical affinity is exerted; he does not however point out very clearly in what manner the chemical action which is exerted by the metals can be connected with the phenomena of galvanism.

In the year 1789, Cavallo published a new edition of his Electricity, and he added a good account of the principal facts in galvanism; an ample history of this science may be also found in the Supplement to the Encyclopædia Britannica.

In this state our knowledge of galvanism continued until the beginning of the year 1800, when Volta made his discovery of the apparatus used called the Galvanic Pile. In the Phil. Transf. for that year is a letter from Volta containing an account of the pile, and a detail of many curious experiments which he had performed with it. This instrument consists of a number of circular pieces of two different metals, laid alternately upon each other, with a piece of moistened pasteboard or skin interposed between each pair. The metals which answer the best for this purpose are zinc and silver, which were found to be the most powerful in exciting the muscular contractions in the former experiments. If the two pieces of metal which form the extremities of the pile be grasped firmly in the hands previously moistened, a shock will be felt through the hands and arms, more or less powerful in proportion to the size of the pile. This shock may be repeated as long as the

the pasteboard between the metals retains its moisture. Volta conceives that this apparatus in every respect resembles the electric organ of the torpedo and gymnotus electricus.

As soon as this discovery became known in England, a variety of experiments were performed with the new apparatus, and many very interesting and important facts were discovered: these are for the most part detailed in Nicholson's Journal, Vol. IV. & seq.

In the 4th vol. p. 174, is a paper written by Mr. Nicholson himself. He begins with a description of Volta's pile, and then relates the results of some experiments which were performed upon a similar one by himself and Mr. Carlisle. By using the revolving doubler they found that the electricity was minus in the silver end of the pile, and plus in the zinc end. Mr. Nicholson proposed that the influence should be permitted to pass from one end of the pile to the other through a tube of water; for this purpose a division was made in the conducting wire, which was composed of copper, and the two ends of it were terminated in a small tube of water. Immediately the wire connected with the silver end of the apparatus began to produce a gas, which was found to be hydrogen, while at the same time the wire connected with the zinc end became oxidated. Recourse was then had to a wire of platina which is not oxidable, and immediately gas began to be evolved from both ends. Upon examining the gases separately, that from the silver end was hydrogen, that from the zinc end oxygen, and they were generated nearly in the proportion requisite to produce water. This experiment, as well with respect to its immediate effects, as with regard to the consequences which may be deduced from it, may, I think, be justly considered as the most important that has occurred since the discovery of oxygen by Dr. Priestley. The electric spark was distinctly visible in these experiments.

Carlisle and Nicholson determine the electricity of the pile, and discover its chemical action, and the decomposition of water.

into hydrogen and oxygen.

Importance of this experiment.

Electric spark visible.

Mr. Cruickshank relates his experiments, in which he made use of the interrupted circuit after the manner of Mr. Nicholson; he caused the influence to pass first through distilled water tinged with litmus, and afterwards through water tinged with Brazil wood. In the first case there was a redness produced by the zinc, and in the second by the silver wire. By submitting a portion of water for a long time to the action of the pile, a sensible diminution was observed in its bulk. He observed

Cruickshank, Effect on chemical tests.

the

the production of hydrogen and the oxidation of the zinc end when he used a communication of copper; and when he employed a wire which was not oxidable, he obtained both hydrogen and oxygen according to the observation of Mr. Nicholson.

Reduction of metals from their solution.

He permitted the influence to pass through a solution of a metallic salt, and found after some time, that the metal began to be revived at the silver end of the apparatus.

Mr. W. Henry decomposes sulphuric and nitric acids, but not the muriatic.

Mr. William Henry, of Manchester, submitted concentrated sulphuric acid to the influence of galvanism: he made use of a wire of platina. Not only the water which always enters into the composition of this acid, but also part of the acid itself, appeared to be decomposed, as the oxygen which he procured was in a larger proportion to the hydrogen obtained than is sufficient to compose water. Nitric acid was also decomposed,

Oximuriatic was deoxygenated.

and the water of muriatic acid. When oximuriatic acid was subjected to its influence, the water was decomposed, and the acid was deoxygenated. Gasses do not appear to be conductors of this influence, so that Mr. Henry was not able to try its effects in decomposing muriatic acid gas. It also appears to have the power of decomposing ammonia.

Gases could not be subjected to it.

Ammonia was decomposed.

(To be continued.)

* * On account of the great Quantity of original Communications, the *Scientific News and Account of Books* is necessarily deferred.

X. Y. Z. is informed that the derivation of chrome or chromium is from *χρῆμα*, color; and was adopted by Vauquelin, because its compounds with oxygen are coloured. See *Philos. Journal*, quarto, II. 444.

*Mr. Rob^t. Jamieson's Observations
on the Formation of Granite &c.*

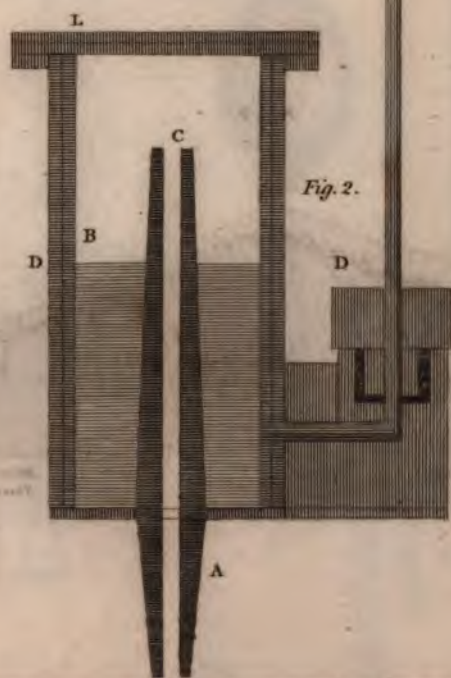
Fig. 1.



Fig. 2.



*M. Bank's Experiments on the velocity
of effluent Air or Gas*



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*Compound Condensers of Electricity:
By W.^r Reed and W.^r Cuthbertson.*

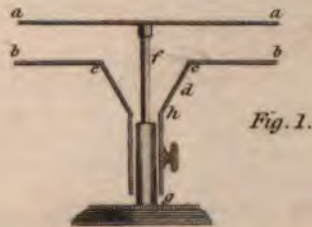


Fig. 1.

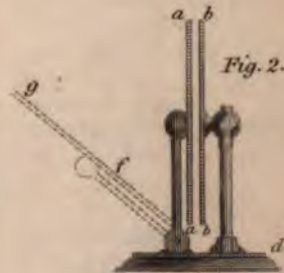
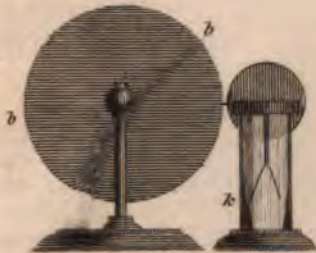


Fig. 2.



Fig. 3.



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INDEX.

A.

- Accum**, Mr. Fred. on the methods by which soda is at present prepared for the English market, 241
- Air**, observations on the velocity with which it issues out of vessels in different circumstances, by Mr. Banks, 269
- Ammonia**, experiments on the production of the muriate of, by Dr. Campbell, 117
- Anhydrous sulphate of lime**, description of, by Comte de Bournon, 190.—Its analysis, by R. Chenevix, Esq. 196
- Atkins**, description of his hydrometer, by J. Fletcher, Esq. 276
- Austin**, Dr. 185

B.

- Bachelay Abbé**, 219
- Bacon**, 10
- Banks**, Mr. on the velocity of air issuing out of a vessel, 269
- Barometer**, remarks on the effects of sound upon the, by Sir H. C. Englefield, 181
- Barraud**, Mr. 227
- Barthold**, 219
- Barton**, Dr. 139
- Barytes**, observations on the figure of the sulphate of, by Mr. H. Sarjeant, 253
- Belidor's hydraulic architecture**, 5
- Bells**, instance of the sudden changes of direction of the sound of, 125
- Bennet**, 174, 285
- Berthollet**, 8
- Best**, Mr. 20
- Blumenbach**, Professor, 291
- Borda**, 252
- Bolton**, 252

VOL. II.

- Boracite**, note respecting the, by Cit. Vauquelin, 120
- Bostock**, Dr. his history of galvanism, 296
- Boswell**, Mr. John Whitley, 1
- Boucherie**, 188
- Bouguer**, 171
- Bournon**, Comte de, his account of stones fallen on the earth, 259.—On the anhydrous sulphate of lime, 190.—On the phosphorescence of the tremolite and Dolomie, 290
- Boyle**, 10
- Boyle**, 165
- Bradley**, Dr. Ez. Walker on his method of observing transits, 22
- Brazil wood**, its qualities, 202
- Brougham**, 166
- Bullock**, Mr. James, 77
- Bullock**, Mr. William, his drawback lock for house doors, 204

C.

- Cachou**, its qualities, 201
- La Caille**, his astronomy, 67
- Caloric**, its effects on imbibed solar light, 104.—Its existence in the oxygen of supporters, 94
- Camac**, Mr.
- Campbell**, Dr. H. on the art of paper making, 6.—His experiments on the production of sulphate of soda, carbonate of magnesia, and muriate of ammonia, 117
- Camphor**, its action on copal, 258
- Canton**, Mr. 106
- Carbonic acid gas**, simple method of decomposing it, 46.—In atmospheric air, 161
- Carcel**, his improvements in a lamp upon Argand's principle, 108

b

Carcau,

INDEX.

Careau, his improvements in a lamp upon Argand's principle, 108
 Carlisle, Mr. 303
 Cavallo, 289
 Cavendish, Mr. 92
 Ceres, the new planet, 20, 48, 54.—
 Account of, by the Baron de Zach, 56,
 213.—Elements of its orbit, 57.—Dr.
 Herschel's communications respecting
 it, 141, 221
 Chladni, 218
 Chaptal, 8
 Chemistry, theory of, observations on the,
 by the Rev. J. Priestley, 69
 Chenevix, Richard, Esq. 192, 293.—His
 account of a peculiar vegetable principle
 contained in coffee, 114.—Analysis of
 the different species of arseniates of cop-
 per, 194.—Analysis of anhydrous sul-
 phate of lime, 196
 Cleaveland, Mr. John, on the art of making
 glue, 235
 Close, Mr. 1, 238.—On the composi-
 tion of writing ink, 149
 Cockchafer, observations on the destruc-
 tion of its grub, by Edward Jones, Esq.
 73
 Coffee, on a peculiar vegetable principle
 contained in, by R. Chenevix, Esq.
 114
 Cold, its effects on imbibed light, 106
 Colours, observations on the theory of,
 by Dr. Tho. Young, 78, 162
 Combustibles, 11
 Combustion, Dr. Tho. Thomson's eluci-
 dations of, 10, 92.—Supporters of,
 their analogy to the products, 93.—
 Remarks on Dr. Thompson's theory of,
 206
 Condenser, description of Mr. Read's
 compound electrical, 284.—Method of
 applying them to the galvanic instru-
 ment, 286
 Copal, advantage of, in the composition
 of ink, 147
 Copper, blue, carbonate of, 195
 Coventry, his micrometers, 166
 Coulomb, his experiments on magnetism,

143.—Formula to determine the mag-
 netic force of a body, 144
 Crawford, 95
 Cruickshank, 70.—His answer to Dr.
 Priestley's memoir in defence of the
 doctrine of phlogiston, 42
 Cathbertson, Mr. John, on Volta's ex-
 periments, upon which he founds his
 theory of galvanism, 281
 Cuvier, Mr. 140

D.

Darwin, Dr. 162.—His zoonomia, 308
 Davis, Mr. 256
 Davy, 116, 164
 Denisard and Deuille invented the pressure
 engine in 1731, 5
 Descroiselles, on the danger of keeping
 phosphorus without particular caution,
 41
 Deuille, one of the inventors of the pres-
 sure engine in 1731, 5
 Disé, 99
 Dolomie, observations on its phosphores-
 cence, by the Comte de Bournon, 290
 Dragon's blood, its qualities, 201
 Drawback lock for house doors, by Mr.
 Wm. Bullock, 204

E.

Echos, deceptions from, 124.—Effect of
 undisturbed, upon the voice or an
 instrument, 126
 Electric fluid, explanation of its action in
 the galvanic pile, 287.—Atmospheric,
 286.—Expansion of carbonated hydro-
 gen by, 184
 Engine, description of a cheap, for raising
 water, by Mr. H. Sargeant, 60
 Engines, hydraulic, improvements of, 2
 Englefield, Sir Henry C. on the effect of
 sound upon the barometer, 181
 English standard, comparison of an with
 the French definitive metre, 244
 Erskine, Mr. 257

Ether,

INDEX.

Ether, table of its colorific undulation, 169
 Ethereal medium, attraction of bodies for the, 85
 Euler, 79, 86

F.

Fabroni, 302
 Falconer, Rev. Tho. his geography of Strabo, 224
 Finery cinder, 234
 Fire, Thomson on, 98
 Fish, luminous, 34
 Fletcher, J. Esq. on Atkins's hydrometer, 276
 Fluor spar, its phosphorescence explained, 99
 Fourcroy, 197
 Fowler, 299
 French and English paper, their relative qualities, &c. 7
 Frost, its use in the crystallization of salts, 219
 Fustet, 202
 Fustic, 201

G.

Gallic acid, 199
 Galvani, his work, 297
 Galvanism, history of, by Dr. Bostock, 296
 Gaseous oxide of carbon, its specific gravity, 43.—Its production from metallic calces, 44
 Gauss, 51, 59
 Glauber's salt, method of obtaining it, 118.—Its use in the preparation of soda, 243
 Glow-worm, experiment on its light, 33
 Glue, account of the art of making, by Mr. J. Cleannel, 235.—Its characters when good, 236
 Goodwyn, Mr. his hydraulic engine improved, 2
 Gough, Mr. John, on ventriloquism, 122.—Answer to his essay on the the-

ory of compound sounds, by Dr. Tho. Young, 264, 267
 Graham, Dr. 139
 Grains of Avignon, their qualities, 202
 La Grange, 210, 265
 Granite, account of, by Mr. R. Jameson, 225
 Gregory, Mr. Olinthus, his treatise on astronomy, 64.—Observations on Mr. Pearson's analogy for deducing the greatest equation, 65
 Gren, 239
 Greville, Rt. Hon. Charles, 258.—His specimen of anhydrous sulphate of lime, 191
 Guillot, Cit. Merat, his reflections upon the art of tanning, 70
 Guyton, Cit. his stove, 24

H.

Hall, Sir James, 230
 Halley, Dr. 49
 Hatchett, Charles, Esq. his analysis of a mineral substance from North America, 129, 176
 Haüy, 174, 197
 Hearing, phenomena of oblique, explained, 124
 Henry, Mr. 185, 186, 304
 Herschel, Dr. 164.—His observations on the new planets, 221
 Hydrogen, carbonated, observations on its expansion by electricity, 184
 Hydrogen gas, sulphurated, its effects on spontaneous light, 101
 Higgins, Dr. 107
 Hifkerdeau, 188
 Hooke, 10, 169.—His explanation of inflection, 172
 Hornblower, Mr. J. C. his observations on the beams of steam engines, 68
 Howard, Edward, Esq. his experiments on stony and metalline substances supposed to have fallen on the earth, 216, 254
 Hulme, Dr. Nath. on spontaneous light, 31, 100

b 2

Humbold,

INDEX.

Humbold, 151
 Hunter, Mr. John, 240
 Hutton, Dr. 67, 229, 331
 Huygens, 76, 87, 91, 173
 Hydraulic engines, 1
 Hydrocarbonates, its combustion in oxygen
 gas, 44
 Hydrometer, description of Atkins's, by
 J. Fletcher, Esq. 276

I.

Incombustibles, 11
 Ingenhouz, 151, 158
 Ink, ancient method of composing, 147.
 —Observations on the composition of,
 by Mr. W. Clofe, 145.—Preparation
 of indelible, by Mr. T. Sheldrake,
 237.—Receipts for the composition of,
 146
 Iron, chromated, its discovery in France,
 64.—Native malleable, 260.—Obser-
 vations on its conversion into steel, by
 Dr. Priestley, 233
 Instrument, description of an, to measure
 the force of the blast of bellows, 272
 Instruments, history of, for shewing mi-
 nute quantities of electricity, 288
 Jameson, Mr. Robert, on granite, 225
 Jefferson, Mr. 140
 Jones, Edward, Esq. on the destruction
 of the grub of the cockchafer, 73
 Jordan, Professor, 63

K.

King, 175, 217
 Kite, incident of a, destroyed by a weazel,
 75
 Klapproth, 192, 197, 293

L.

Lachenau, Haffel, his new process for
 claying sugars, 187
 Lalande, 48, 52.—His astronomical prize,
 141
 Lamp, description of one upon Argand's
 principle, by Citizens Carcel and Ca-
 seau, 108

Landriani, Chevalier, 63
 Lange, Citizen, 109
 Lavoisier, 10, 70, 95
 Lenoir, 248
 Lethenyey, Captain, 63
 Lewes, Mr. 107
 Light, emitted from bodies, Dr. Hulme
 on, 31.—Observations on the theory of,
 by Dr. T. Young, 78.—Answer to
 Newton's objections to the undulatory
 system of, 90.—Its existence in com-
 bustibles, 94.—Experiments on the
 spontaneous emission of, from various
 bodies, by Dr. Nathaniel Hulme, 100.
 —Remarks on the theory of, by Dr.
 Young, 162

Lime, its precipitation by an infusion of
 tan, 71

Lithophylacium Bornianum, 258

M.

Maclane, Mr. 257
 Magee, 166
 Magnesia, experiments on the production
 of the carbonate of, by Dr. Campbell,
 117
 Magnetism, its galvanic effects, 62.—
 Experiments on, by Citizen Coulomb,
 143
 Mamoth, remarks on the, by Louis Va-
 lentine, 138
 Mandrepore, formation of, 255
 Mayow, 10
 Measures, tables of the French, 250
 Melfer, 58
 Metal, new, 178, 180
 Meteor, account of the explosion of a,
 near Benares, 255
 Metre, comparison of the French defini-
 tive, with an English standard, 244
 Meyer, 98
 Michell, 170, 174, 227
 Mill for grinding hard substances, by Mr.
 G. Terry, 206
 Mineral substance, analysis of a new, by
 Charles Hatchett, Esq. 129
 Mineral, analysis of a, from North Ame-
 rica, by Charles Hatchett, Esq. 176

Mineral,

INDEX.

Mineral, from North America, its composition, 177
 Moles, their general habits, 74

N.

Newton, 79, 168.—His objections to the undulatory system of light, 89.—Principia, 88.—Theory of the law of undulations, 266
 Nicholson, 304.—On Volta's pile, 303
 Nitrous gas, its effects on spontaneous light, 103
 Nomenclature, remarks on, 212

O.

Olbers, his discovery of a new planetary body, 20, 58
 Ore, from North America, description of, 130, 132
 Oxigenation is not the same act as combustion, 11
 Oxygen gas, tables to shew the difference in the quantity and quality of, obtained from plants exposed to solar light in river water, and water impregnated with carbonic acid, 154, 156

P.

Pallas, the new planet, remarks on, by Baron de Zach, 213.—Elements of the orbit of, 214.—Ephemeris of the position of, 215.—Dimensions and nature of, by Dr. Herschel, 221
 Paper-making, remarks on by Dr. H. Campbell, 6
 Pearson on the new planet Ceres, 48.—his analogy for deducing the greatest equation, observations on, by Mr. O. Gregory, 65
 Pelletier, 98
 Phlogiston, answer to Dr. Priestley's defence of the doctrine of, by Mr. W. Cruickshank, 42
 Phosphorescence, cause of, in the tremolite and dolomie, 292

Phosphorus, caution against the usual method of keeping it, 41.—Experiments on Canton's, 105.—Its acidification in nitrous acid, 209

Piazzi, Mr. 56
 Picket, 164, 246, 248
 Pigott, Mr. 182
 Piston, elastic, 6
 La Place, 95
 Planet, new, discovered by Dr. Olbers, 20
 Plants, experiments on the vegetation of, by James Woodhouse, M. D. 150.—Effects produced by their growth in atmospheric air, 152.—By their leaves, exposed to solar light in a mixture of atmospheric and carbonic acid gas, 153.—Objection to the hypothesis of their emission of oxygen and absorption of azote, 159.—Tables to shew the effects of their leaves when exposed to solar light, 160

Playfair, Professor, 219, 231
 Pontier, 64
 Pressure engine of Trevithick, 5.—Invented in 1731, ib.
 Priestley, Dr. his method of decomposing carbonic acid gas, 46
 Priestley, Rev. J. his observations on the theory of chemistry, 69, 136, 158.—On the conversion of iron into steel, 233
 Prony, Mr. 246
 Proof spirit, its specific gravity, 277
 Proust, 72, 195, 116.—On tannin, 198, 200
 Pump, method of applying a temporary forcer to a, by Mr. Richard Trevithick, 216
 Putrefaction, observations on the causes why a large quantity of salt prevents, and a small quantity hastens, 239
 Pyrites, account of the, found in the stone from Benares, 260

R.

Bags, for paper, valuable observations on,

INDEX.

Read, Mr. John, his condenser, 283
Robertson, 67
Robison, Professor, 39, 300
Roy, General, 245
Rumford, 158, 164, 175

S.

Sarjeant, Mr. H. his cheap engine for raising water, 60.—On sulphate of barytes, 253
Sausfure, M. de, 291
Scheele, 8, 97
Schemnitz, hydraulic engine of, improved, 1.—Compared with Goodwyn's, 3
Schroeter, Dr. J. H. his letter respecting the new planet of Olbers, 20, 58, 142
Science, account of books, 223
Seeds, effects produced by the germination of, in atmospheric air, 151
Seguin, 116
Selkirk, Earl of, 230
Senebier, 96
Sheldrake, Mr. Tho. on the preparation of indelible ink, 237
Short, 164
Shuckburgh, Sir George, 245
Silk buried in the earth, remarks on, by the Rev. T. Wilson, 222
Smith, Mr. 179.—Dr. 265
Soda, experiments on the production of sulphate of, by Dr. Campbell, 117.—Account of the method by which it is at present prepared for the market, by Mr. Accum, 241.—Its value, 242.—Method of preparing it in Prussia, 243
Sound, deflection of, 91.—Diagram to shew the effects of, on the head, 123. Its effects on the barometer, by Sir H. C. Englefield, 181
Southey, Mr. 218
Spedding, James, Esq. his engine for raising water, 61
Stahl, 18
Steam engines, observations on the beams of, by Mr. J. C. Hornblower, 68
Steam, apparatus for heating water by means of, by Mr. Arthur Woolf, 203

Stones, experiments on, supposed to have fallen on the earth, by Edw. Howard, Esq. 216, 254
Stove of Guyton, 24
Stromayer, Mr. 121
Sugars, new process for claying, by Cit. Hassel Lachenaie, 187
Sumach, its qualities, 202
Supporters of combustion, 11

T.

Tanin, Memoir on, by M. Proust, 198
Tanning, observations on, by Cit. Merat Guillot, 70
Tellurium, account of, 62
Terry, Mr. Garner, his improved mill for grinding hard substances, 206
Thomson, Dr. Tho. his improvements in the doctrine of combustion, 10, 92, 151.—Remarks on his theory of combustion, 206
Time Pieces, irregularities of diminished, by Mr. Eackiel Walker, 70.—On the variation of rate in, by Mr. E. Walker, 273
Tremolite, on its phosphorescence, by the Comte de Bournon, 290
Trevithick, Mr. Richard, 5.—His method of applying a temporary forcer to a pump, 216
Troughton, his use of spider's webs in the foci of telescopes, 23.—His comparer, 247
Tschacky, Major, 63

V.

Vacuum, effects of on spontaneous light, 104
Valentine, Louis, his remarks on the Mammoth, 138
Valli, 298
Vauquelin, 192, 199.—On the boracite, 120
Ventriloquism, observations on, by Mr. John Gough, 122
Vibrations, table of, 274

INDEX.

Vince, his astronomy, 67

Vinci, Leonardo da, his treatise on painting, 142

Volta, examination of his experiments, by Mr. J. Cuthbertson, 281, 298

U.

Undulations, lateral, 88, 163

W.

Walker, Mr. W. on the new planets, 21

Walker, Ez. his method of obviating the effects of the wire in transits, 22.—On the irregularities of time pieces, 76.—On the variation of rate in a time piece, 273

Ward, his elliptic hypothesis, 49

Water, apparatus for heating, by waste steam, by Mr. Woolf, 203

Weights, tables of the French, 251

Wells, Dr. 301

Werner, 228, 232

Williams, Mr. 255

Wilson, Rev. James, on silk buried in the earth, 222

Winthrop, Mr. 130

Wistlar, Dr. 139

Wood, rotten, its luminous appearance, 33

Woodhouse, Dr. James, his experiments on the vegetation of plants, 150

Woolf, Mr. Arthur, his apparatus for heating water by waste steam, 203

Y.

Young, Dr. Thomas, on light and colours, 78, 162.—On the effect of sound upon the barometer, 183.—His revision of the comparison of the French metre with the English standard, 249. Answer to Mr. Gough's essay on the theory of compound sounds, 264.—Corrections in his paper on the mechanism of the eye, 268

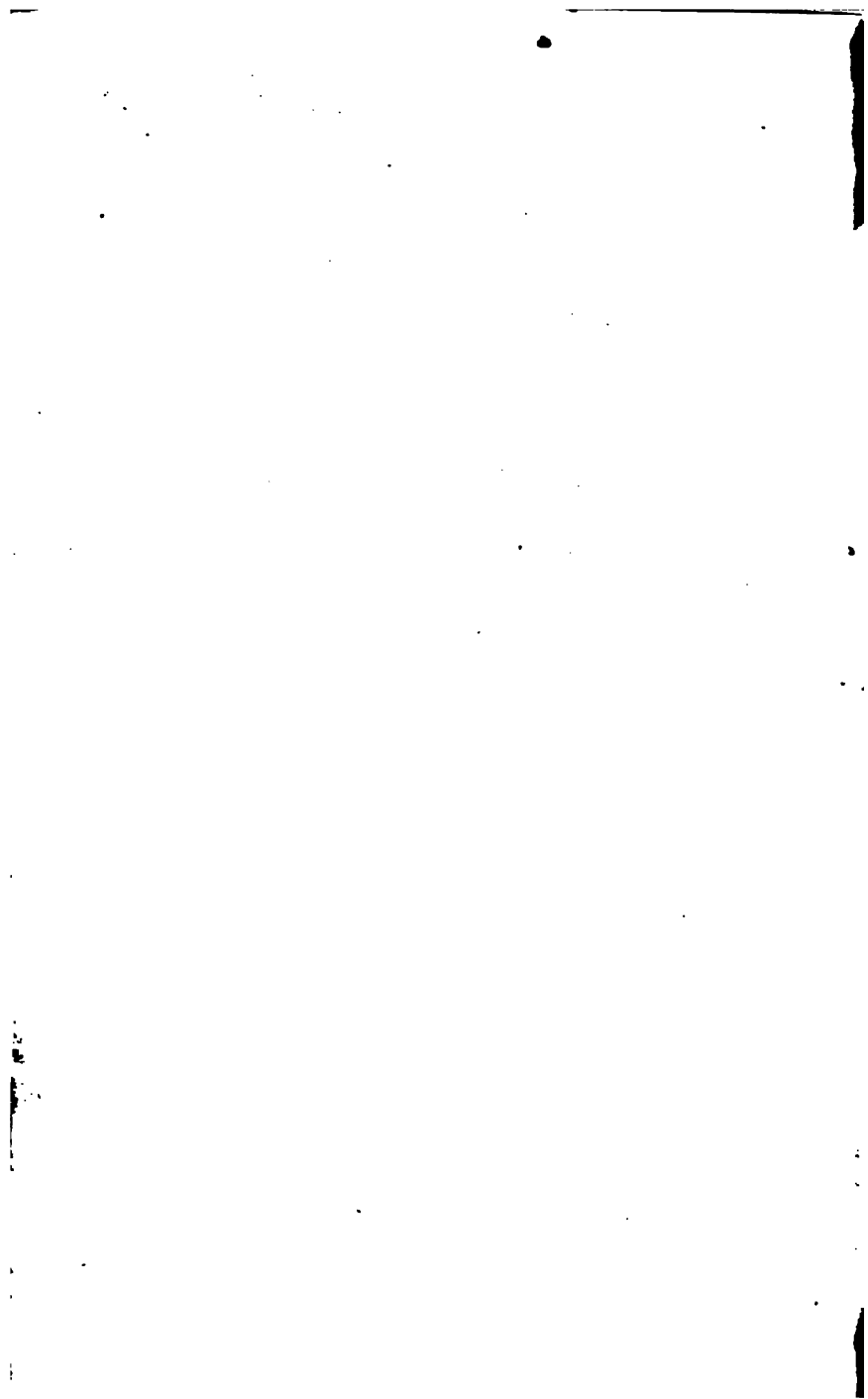
Z.

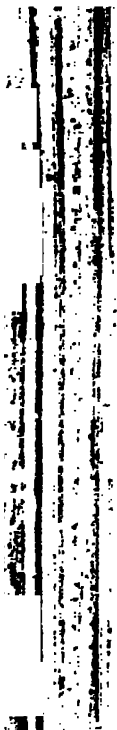
Zach, Baron de, his observations on the new planet Ceres, 56.—And Pallas, 213

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